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ABSTRACT

The object of this theoretical inquiry is to examine the controversial issue of a fully automatic high quality translation (FAHQT) in the light of past and projected advances in linguistic theory and hardware/software capability. This first volume of a two-volume report discusses the requirements of translation and aspects of human and machine translation. Techniques involved in machine translation, i.e., hardware, software, and linguistics, are covered. Contributions of linguistics to machine translation and pertinent recent work in linguistics are also considered. Views of specialists concerning machine translation are presented along with the conclusions and recommendations of the report. An appendix includes relevant papers contributed by specialists on the topic of machine translation. Papers are also included in the second volume of the study. For Volume 2, see FL 002 937. (VM)

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Final Technical Report
December 1971

FEASIBILITY STUDY ON FULLY AUTOMATIC HIGH QUALITY TRANSLATION

University of Texas

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
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FEASIBILITY STUDY ON FULLY AUTOMATIC HIGH QUALITY TRANSLATION

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FOREWORD

This is the Final Report, in two volumes, for the Feasibility Study on Fully Automatic High Quality Translation, by the University of Texas, Linguistic Research Center, Austin, Texas, for Rome Air Development Center, Griffiss Air Force Base, New York, under contract F30602-70-C-0129, Job Order No. 45940000. Zbigniew L. Pankowicz (IRDT) was the RADC Project Engineer.

As the appendices indicate, the study brought together specialists in the areas involved in machine translation. The report summarizes their findings. Participants in the study were provided with a preliminary statement of the initial part of this report, except for the conclusions and recommendations, and were asked to send their comments and revisions. These were incorporated in this report, except when they did not seem in keeping with the general conclusions of the various other participants. There were few strikingly diverse points of view.

The authors are grateful to the participants, and to the sponsors who made the study possible.

This report has been reviewed by the Information Office (OI) and is releasable to the National Technical Information Service (NTIS).

This technical report has been reviewed and is approved.

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ABSTRACT

This report presents the results of a theoretical inquiry into the feasibility of a fully automatic high quality translation (FAHQT), according to Bar-Hillel's definition of this term. The purpose of this inquiry consisted in determining the viability of the FAHQT concept in the light of previous and projected advances in linguistic theory and software/hardware capabilities. The corollary purpose was to determine whether this concept can be taken into consideration as a legitimate and justifiable objective of R&D. The effort was supported by 20 expert consultants from the various universities and research centers in the U.S.A. and abroad. Conclusions and recommendations are presented on pages 44-50 of the report. Individual contributions of participants and consultants reflect a wide range of opinions concerning the prospects of FAHQT in intermediate and long range of R&D.

TECHNICAL EVALUATION

The objective of this theoretical inquiry is to examine the controversial issue of a fully automatic high quality translation (FAHQT) in the light of the past and projected advances in linguistic theory and hardware/software capability. The principal purpose of this study is to determine whether the concept of FAHQT is justifiable as a long range R&D proposition. The study is also concerned with the intermediate range alternatives to FAHQT, i.e., machine translation forms that are adequate to the user's needs with or without post-editing. Machine aided translation, based on the automated dictionary look-up, is excluded from the study in consideration of the fact that this by-product of machine translation R&D is well within the current state-of-the-art.

In the context of FAHQT, "full automation" implies that the entire translation process is autonomous in the computer without pre-editing of the source language text and post-editing of the target language output. "High quality" seems to be undefinable in an absolute sense. In referring to machine translation of 100% quality, Bar-Hillel (1) introduced the following qualification.

> "When I talk about "100%", I obviously have in mind not some heavenly ideal of perfection, but the end product of an average human translator. I am aware that such translator will on occasion make mistakes and that even machines of a general low quality output will avoid some of these mistakes. I am naturally comparing averages only".

Thus viewed, even the concept of 100% quality is not equatable with the error-free performance in either form of translation. Understandably enough, participants and consultants failed to reach a unanimous agreement as to the definition of "high quality" in machine translation. This is reflected on p. 48, quote, "There is apparently no absolute standard. Rather, standards must be defined with reference to specific users and specific purposes". In the absence of absolute and universally valid quality criteria, the user of machine translation can be legitimately considered an ultimate judge of its quality. This viewpoint was first expressed by Reitwiesner and Weik (2) as early as in 1958.

According to Lamb (3), "all translation can be viewed as human translation since machine translation is nothing but another kind of human translation". It follows from this observation that the fundamental constraints on machine translation parallel those imposed on human translation. Assuming the well-known limits of translatability, this seems to imply that either form of translation is a priori constrained. In summarizing the problem of translation equivalence between SL (source language) and TL (target language), Catford (4) draws the following conclusion.

> "The limits of translatability in total translation are, however, much more difficult to state. Indeed, translatability here appears, intuitively, to be a cline rather than a clear-cut dichotomy. SL texts and items are more or less translatable rather than absolutely translatable or untranslatable. In total translation, translation equivalence depends on the interchangeability of the SL and TL texts to (at least some of) the relevant features of situation-substance".

Ray (5) recognizes the fact that "every translation necessarily involves some distortion of meaning". However, as is reflected in his statements below, this deficiency is not only manageable, but even unimportant in the practice of translation.

> "The translation operation is, like the limit operation, possible only under such conditions as "sufficiently" and "arbitrarily", that is, only by the exercise of some evaluative judgement, however little. Since distortion of meaning cannot be avoided, the problem becomes one of confining it to allowable measures of allowable kinds in allowable places along allowable directions".
>
> "..., while no two languages will match exactly in the total range of possible discourse, there are infinitely many specific limited ranges of discourse where the distortion of meaning can be legitimately dismissed as of no account".

The feasibility of FAHQT must be, therefore, considered within the limits of translatability, i.e., taking into account the constraints on the total translation. Since the concept of high quality is untenable in the absolute sense, the question of what is feasible in the context of FAHQT is quite probably more meaningful. It would be patently unreasonable in this stage of R&D to postulate machine translation requirements beyond the limits of translatability imposed on human translation.

Machine translation research, based on puristic notions and oriented toward a global solution, was once compared to a search for the Holy Grail. This all-or-nothing attitude has probably caused as much damage to the progress of machine translation research as the early announcements of quick and easy solutions. Perfectionists in this area have generally tended to ignore the injunction by Lecerf (6) that "entreprendre la mise au point d'ensembles de traduction automatique, c'est avant tout accepter la contrainte du reel".

According to Ljudskanov (7),

> "The widespread so-called 100 percent approach, along with the belief that MT presupposes the presence of a complete mathematical model of language in general and of the specific languages in particular, in practice amounts to equating the nature and extent of the knowledge of language in general, which is necessary from the point of view of theoretical linguistics, with the extent of knowledge necessary for the achievement of translation from one language into another. This approach also amounts to equating the description of communication in general with that of the translation process; it ignores the specific characteristics of the process as mentioned above and the general linguistic problems of the theory of translation (both HT and MT) in the general problem area of mathematical linguistics".
>
> "....it can be asserted that the current critical state of MT research throughout the world, although much has happened that legitimately causes well-grounded anxieties and doubts as to its possibilities, is due to a certain degree to the maximalistic tendencies, however laudable they may be in themselves, of the global strategy. By giving due consideration to the particular characteristics of the translation process and of its study, as well as to the differentiation of the aims of mathematical linguistics from the theory of MT and of the fields of competence and performance from each other, research in this field would be channeled in a direction both more realistic for our time and more closely in accord with the facts".

The report highlights on p.4 an important, but often ignored, difference between scientific and technical translations and translations of literary and religious texts, in spite of its importance from the viewpoint of machine translation requirements.

> "Even articles and monographs dealing with machine translation have failed to be adequately explicit about the special problems of translating technical and scientific materials by computer. Instead, they have confused the problem by comparing machine translation with the long-practiced human translation, by equating the problems of translating scientific materials with those involved in translating literary materials, and by using the same evaluation criteria for the results".

It is now a commonplace that the style of writing is of paramount importance in literary translation, whereas the accuracy constitues the most important quality criterion in scientific and technical translations. According to Gingold (8),

> "It is not the translator's job to abstract, paraphrase, or improve upon the author's statements. He cannot be expected to convert an article that is poorly organized and badly written in the original language into a masterpiece of English scientific writing. In technical translation, he must always be willing to sacrifice style on the altar of accuracy".

Savory (9) has expressed a similar opinion in his statement that "the translation of scientific work is an ideal example of translation of a writing in which the subject matter is wholly on the ascendant and the style is scarcely considered".

The report further emphasizes the crucial importance of timeliness in production of scientific and technical translations. According to the statement on p. 5, "...timeliness is of increasing importance to users of scientific translations. Even in a relatively unhurried field like linguistics, few articles retain their importance over a long period. Statements have been made repeatedly about the obsolescence of publications issued a few years earlier. The insistence among technical specialists and scientists for speedy translation contrasts markedly with the length of time permitted for completing literary translations". The requirement of timeliness was stressed elsewhere by Gingold (10), quote, "The delay between the appearance of the original journal and its English translation, which may be a year or more, is also a disadvantage, particularly to industry, where time is usually of great importance".

The principal findings of the study, as related to its objectives, can be summarized as follows.

Computer hardware is no longer considered a crucial problem in machine translation. "Remarkable improvements, especially in rapid-access storage devices, have largely eliminated the problems caused by inadequate computers. Lexical items can now be retrieved as rapidly as were the major syntactic rules a decade ago. And with further improvements of storage devices in process, computers no longer pose major problems in machine translation". (p. 12). Developmental prospects in this area are very bright indeed, particularly with the advent of holographic memories. The impact of such memories on both linguistic and computational aspects of machine translation R&D is discussed in detail by Stachowitz in one of his contributions to the report ("Requirements for Machine Translation: Problems, Solutions, Prospects", pp 100-106). This contribution is considered significant because it provides a complete blueprint for a realistic implementation of a large-scale machine translation system.

Equally encouraging is the appreciation of advances in computer software. "Programming has evolved as rapidly as have computers... A key factor here was the enrichment of programming language data types which made possible efficient representation and manipulation of linguistic structures". (p. 13).

The report reflects a unanimous agreement of participants and consultants that "the essential remaining problem is language" (pp 14-15). It is, therefore, not surprising that linguistics has received much more attention in the study than computer hardware and software. Recommendations presented on pp 49-51 are exclusively oriented toward linguistic research in the context of machine translation.

The report points out that there is "no conflict between specialists in descriptive linguistics, linguistic theory and machine translation... As descriptive linguists improve their understanding of language, and the models by which to express that understanding, machine translation specialists will update their procedures and models". (p. 24). However, the report also reflects a difference of opinions between machine translation experts and linguists as regards the nature, orientation and scope of linguistic research involved in machine translation. It is further worth noting that some linguists participating in this study have not acknowledged Ljudskanov's caveat about "maximalistic tendencies of the global strategy".

The reader is referred to Conclusions (pp 45-48) and Recommendations (pp 49-51), summarizing the results achieved in performance of this study. Recommendation of support for research in machine translation is based on the fact that "quality translations can be achieved in the near future. This recommendation agrees strikingly with conclusions reached in a study carried out in the Soviet Union". (p. 49). Galilei's challenge ("Eppur si muove!"), aptly chosen as a motto in the Introduction to (11) by Kulagina and Mel'chuk, would be equally appropriate as an expression of views and sentiments embodied in the main part of this report.

ZBIGNIEW L. PANKOWICZ
Technical Evaluator

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1. Requirements of translation

With the increase in communication and in publication, translating has achieved a greater importance than ever before. Literary figures are engaged in translating from many exotic languages, as well as from the traditional languages of western culture. Symposia on translation have been held, resulting in the publication of monographs on the topic. Above all, scientists and technical specialists have come to demand translations. As one of the leading experts, Eugene A. Nida, has stated in his most recent contribution to the topic (Nida and Taber, 1969, 1): "Never before in the history of the world have there been so many persons engaged in the translating of both secular and religious materials." The book intimates that the requirements for translation will be increased.

Moreover, it describes more specifically and concretely than earlier discussions the steps that are involved in translation. Translation is defined (Nida and Taber, 1969, 12) as "reproducing in the receptor language the closest natural equivalent of the source-language message." And the paragraph continues: "this relatively simple statement requires careful evaluation of several seemingly contradictory elements."

For a fuller statement on the problem of translation, we refer to the important books by Nida and their bibliographies. His last book, however, contains further perceptive statements that are important to include here.

A section on "the old focus and the new focus" of translating (Nida and Taber, 1969, 1) states that "the older focus in translating was the form of the message...The new focus, however, has shifted from the form of the message to the response of the receptor."

Further, "even the old question: Is this a correct translation? must be answered in terms of another question, namely: For whom?" After a brief answer, the section continues: "In fact, for the scholar who is himself well acquainted with the original, even the most labored, literal translation will be correct, for he will not misunderstand it." This statement is borne out by the reception to such translations at Oak Ridge, as reported by Zarechnak below.

The growing sophistication with regard to translation which is reflected in the book by Nida and Taber and in many recent publications calls for a new evaluation of the problem of machine translation, and a new statement on the current situation. The requirements for translation vary markedly from audience to audience. Even a glance at the Nida-Taber book, which concerns primarily human translations of the Bible, will disclose the difference between translation of religious and literary materials, and translation of scientific and technical materials.

For the translation of technical materials, the criteria of quality, speed, and cost have been used in evaluations. In the January Conference arranged under the Study, Bar-Hillel summarized his position on the improvements possible in machine translation in the foreseeable future using these three criteria. It is instructive to compare briefly these criteria with the objectives of Nida-Taber.

The primary concern of Nida-Taber is to "reproduce the message" of texts produced by cultures of the past for cultures of the present, often radically different cultures, such as those of Africa and Asia. By contrast, the texts of interest to scientists and technicians share a common "culture," whether the texts are produced in Africa, Asia or in western countries. Zarechnak, who as director of Oak Ridge Russian-English translation is intimately acquainted with user needs of scientific translation, reports that American scientists readily understand translations of Russian scientific articles even when these are crudely rendered into English. Translations of technical materials accordingly face far fewer problems than do literary and religious translations.

This fact, obvious in any chapter of Nida-Taber, has often been disregarded when the requirements for technical and scientific translation have been discussed. Even articles and monographs dealing with machine translation have failed to be adequately explicit about the special problems of translating technical and scientific materials by computer. Instead, they have confused the problem by comparing machine translation with the long-practiced human translation, by equating the problems of translating scientific materials with those involved in translating literary materials, and by using the same evaluation criteria for the results. In his appended article Martin discusses the problem confronting the human translator of non-scientific material with respect to differences in cultural associations. The example he provides illustrates that some of these problems may not be encountered in technical and scientific materials.

Among the most striking contrasts are the disregard of cost and time in translating religious materials, as indicated briefly below.

The Appendix to Nida-Taber (1969, 174-188) deals with the "organization of translation programs." An organization consists preferably of three committees: (175) "1. the Editorial Committee, which has the basic responsibility for the work of translation,

2. the Review Committee, consisting of highly competent scholars whose advice and help is necessary, and

3. the Consultative Group."

After these three committees have made their contribution, a "stylist is called in" (1969, 186). This proposed organization, which is not untypical for academic projects designed to produce literary translations, provides perspective for the statements concerning post-editing of technical and scientific translations. Obviously, the length of time and the cost required to produce literary and religious translations are not factors of importance.

Yet timeliness is of increasing importance to users of scientific translations. Even in a relatively unhurried field like linguistics, few articles retain their importance over a long period. Statements have been made repeatedly about the obsolescence of publications issued a few years earlier. The insistence among technical specialists and scientists for speedy translation contrasts markedly with the length of time permitted for completing literary translations, and also with "the lag time (from receipt) in publication of the translated journals supported by NSF." This, according to a report of the National Academy of Sciences, (Languages and Machines, 1966, 17) "ranges from 15 to 26 weeks." This time span may be acceptable for archival purposes; for the requirements of scientists and technical specialists it may be burdensome.

Given a choice between overnight machine translation and human translation within two weeks, scientists at EURATOM invariably asked for machine translation. The need for virtually immediate translation is one of the major reasons for the concern with machine translation. In evaluating machine translation versus human translation, this reason may outweigh the difference in cost. And as Nida has pointed out, the parameter of "quality" varies considerably among the different users. Bar-Hillel, who some years ago coined the expression "High Quality Fully Automatic Machine Translation" now states in this appended article that he applied the expression in too absolute a sense. Further, that quality is related to the requirements of the user. This statement echoes the quotation from Nida-Taber on the shift of focus "from the form of the message to the response of the receptor." If technical experts and scientists have reasonable prospects of virtually immediate translation, the prospects may well be vigorously pursued, even if the translations will be more "labored" and "literal" than ordinary users permit for their religious and literary works.

In reviewing the prospects for machine translation, accordingly, the specific requirements must be considered as one of the major criteria. For technical specialists and scientists, translations must be consistent, reliable and timely, whether made by man or machine. Although the arrangements made for human translation are generally assumed to be known, and understood, a brief comparison of the current situation of human versus machine translation, and their prospects, may be useful before examining in detail the procedures involved in machine translation.

2. Translation: human and machine

The topic of machine translation is rarely discussed without reference to translation by man. In the comparison, several stereotypes have evolved. For clarity in dealing with the issue of machine translation these may be briefly noted.

The human translator is generally assumed to be highly skilled, both in the subject matter and in the source and target languages. Some commentators consider skill in the source language less essential than skill in the target language. Kay holds knowledge of the subject matter to be the most essential consideration. Accordingly, it will be no small task to provide machine translation systems with detailed information on scientific topics, and to program them to use this information. Human translators must also acquire knowledge of specific scientific and technical areas. With skills in the source and target languages, and control over the subject matter, the human translator is assumed to have great flexibility. Moreover, besides flexibility he provides immediate access to the text.

When, however, one considers the broad scope of scientific writing, and vocabulary, this ideal picture loses some of its attractiveness. The director of the translation service of the German government states flatly that no available dictionary is up-to-date. A translator dealing with German, one of the most thoroughly studied languages, would be unable to find any translation for thousands of technical terms. Others would have inadequate entries. Accordingly, technical terms might well be wrongly translated. A few moments of reflection by any specialist illustrates this problem of human translators.

One of the chief problems for any translator has to do with the changing meanings of supposedly standard technical terms. A simple area of linguistics, for example, is commonly subsumed under the terms: phonology, phonetics and phonemics. The term phonetics is generally used consistently. But among different authors the terms phonology and phonemics vary widely in their meanings. When such terms occur in texts translated by translation services, they are handled as though they had standard, fixed meanings. But for some writers phonology is used as equivalent to the term phonemics in other writers. Similar illustrations could be provided from any technical area, and all too easily supported by examples.

Without diminishing in any way the role of the human translator, we must conclude that adequate translation requires the organizational arrangements created to meet other contemporary technical problems. As noted above, an example of such an organization is proposed by Nida-Taber. If for a relatively unhurried problem in translation, teams of specialists are recommended, rather than individual translators, it is unrealistic to assume that an individual translator can deal with a broad scope of technical material. Moreover, even skilled human translators need retraining in expanding fields of science and technology if they are to keep up with new terms and new concepts.

To meet the problem, the German translation service has been compiling a large dictionary of technical terms and their standard translation. In this compilation, specific translations are fixed. The project, accordingly, is designed to standardize and normalize translations, as well as to provide assistance for human translators. Moreover, the dictionary is mechanized.

Eventually, any text to be translated is to be provided to the translator in a print-out having the translations of all terms in the dictionary, as well as the original. The translator's responsibility would then consist in framing the sentences in the target language. He would also determine the meanings of any new terms. In this way the dictionary would be expanded and updated.

The dictionary of the German translation service contains close to a million items. Problems which human translators face when using generally available dictionaries, which have far fewer entries, may be put in perspective by this resource. The arrangements for translators in the German translation service may also illuminate the requirements for computer-assisted translation.

It is occasionally proposed that computer-assisted translation is an attainable compromise, with better output than that from the individual translator and fewer awkward renditions than those provided by machine translation. Whatever one's reaction to this view, it should be noted that computer-assisted translation requires a large staff of research scholars, and a large computer facility. Kay, a proponent of machine-human translation, proposes an elaborate scheme to permit human beings to assist a system that is essentially a machine translation system. Under this scheme human beings would make decisions which the machine would be incapable of making and thus assure a high-quality output. His scheme envisions several native, possibly monolingual speakers of the source language, several monolingual speakers of the target language and one highly competent bilingual, to whom problems requiring knowledge of both languages would be shunted. In other words, the expenditure for staff and equipment would not be small, actually larger than that for machine translation. Clearly, computer—assisted translation is proposed as a second choice, through desperation that machine translation is unattainable at present.

In contemplating machine translation, most observers have stereotypes which are as erroneous as are the stereotypes concerning human translators. The output of machine translation is supposed to be simply a printed document of some sort. Consideration of current computer technology however suggests a more likely output of a different kind. Many users of computers already have available display possibilities for research purposes, CRT's. If such research workers wish to secure a translation, little ingenuity would be required to provide it with great effect on the CRT, as described below.

The current arrangements at Oak Ridge, as described by Zarechnak in the January Conference, illustrate the potential output. Scientists at Oak Ridge who request a translation have the text keypunched and mechanically translated when computers are not fully utilized, typically at night. The translations are then available for them the following morning.

If, however, instead of a print-out, the translations were prepared for a running display on CRT's, both the original and the translation would be provided on the screen. The scientists would then be able to consult the original as well as see the translation. Tables and charts, often the most expensive sections to reproduce in translation, would need little attention under such a system. Zarechnak cited an instance at Oak Ridge where a scientist detected an error by referring to a picture which accompanied the translation from the Russian. It turned out that the Russian original contained the error which was carried over into English. The availability of pictures, tables and charts thus provides a check on accuracy. Other advantages of displays of translations on CRT's will not be pursued further here. One obvious advantage is the speed with which the translations would be provided.

Among the advantages of machine translation is consistency. As in the German translation service, standard terms could always be produced. As a simple example, the German translation service decided to use Telefon rather than Fernsprecher; even the variant Telephon was considered erroneous. In much the same way, any technical term need never be varied, unlike the practice of many translators.

If the quality of such translations is to equal that of the most accurate human translations, a comprehensive dictionary and grammar are essential, as well as the necessary hardware and the software techniques. Achieving these has been the major goal of machine translation. In the next section we note the current status of these three requirements.

3. Techniques involved in machine translation:
hardware, software, linguistics

In the early attempts at machine translation, the capacities of computers were a major problem. Difficulties resulted especially from the inadequacy of rapid-access memories. For processing languages, the available rapid-access storage space was filled with the major rules for grammatical constructions. Lexical items accordingly had to be stored in memories, usually on tapes, which required a considerable period of searching. As a result, even simple sentences required a long time for analysis. When the Linguistics Research Center was carrying out its research with an IBM 7040, several years ago, the computer would "grind" all night to translate a few sentences.

To speed up the process, attempts were made to develop special-purpose computers, used only for machine translation. A notable example was the Mark II. Intended for only a short period of use, it was long maintained because of the inadequacies of general purpose computers for data processing, including the procedures required for machine translation.

Remarkable improvements, especially in rapid-access storage devices, have largely eliminated the problems caused by inadequate computers. Lexical items can now be retrieved as rapidly as were the major syntactic rules a decade ago. And with further improvements of storage devices in process, computers no longer pose major problems in machine translation. Proper management can assure the necessary space for storing large sets of lexical and syntactic rules. Skillful use of them depends in great part on recent software developments.

Programming has evolved as rapidly as have computers. The early higher level programming languages were designed largely for application to numerical analysis oriented problems. Linguists who used computers were compelled to write their own assembly language programs. Eventually, in response to the needs of computational linguists, procedural languages such as COMIT were developed; in addition, other higher-level symbol manipulation languages such as LISP and SNOBOL proved useful for linguistic applications in mind. A key factor here was the enrichment of programming language data types which made possible efficient representation and manipulation of linguistic structures.

In time, procedure-oriented languages were used to produce programs of more general usefulness to linguists. Dictionary lookup and maintainance programs and context-free grammar parsing programs were followed by such programming systems as J. Friedman's transformational grammar tester and S. Petrick's transformational grammar syntactic analyzer. Systems such as these can be considered to be problem-oriented programming languages. The IBM natural language question answering project mentioned by Petrick in his appended paper uses Friedman's grammar tester system as well as a transformational syntactic analysis system that provides for a linguistically more realistic class of transformational grammars than could previously be accepted using its predecessor.

Certainly presently existing procedural and problem-oriented languages make the mechanization of many linguistic processes easier than was the case a few years ago. The programming of many linguistic algorithms remains a slow and difficult task, however, as is the case for most complex algorithmic processes.

The scope of the programs necessary for machine translation may be noted by examining a flow-chart of the programs that had been projected, and in part completed, at the Linguistics Research Center. These were produced entirely from scratch. Because the basic programs furnished by computer manufacturers were so inadequate, the Linguistics Research Center programs were written in machine language. The expenditure of time was enormous. The magnitude of the problem may be noted if one compares the cost incurred by IBM in developing PL-1; the cost of it far surpasses the entire amount which was spent on machine translation from the beginning of machine translation research.

Gradually, adequate computer programs were devised for data processing. These now form the basis of programming systems used for machine translation. Like all programs, they need modification, and improvement, especially to speed up processing. The basic programs, however, are available and to them the additional programs needed for language processing can be added. Like computer equipment, programming systems will be improved. But in the same way, they no longer provide an obstacle to work in machine translation. Kay, an authority in the field of machine translation software, states flatly that "the real problems of machine translation are not in program writing; they are in linguistics."

All participants in the study agreed that the essential remaining problem is language. Language has proved to be immensely complex, far more complex than linguists themselves had recognized. Moreover, as noted below, linguists do not agree on what constitutes complexity in language.

The fundamental linguistic problem for machine translation is often discussed in terms of deep or underlying structure. Numerous examples have been given to illustrate the problem; among the most commonly cited are the sentences:

1. He is easy to please
2. He is eager to please

Both sentences are alike on the surface. Yet their meanings differ, as the paraphrases indicate:

1. Someone pleases him readily
2. He pleases someone else with alacrity

Speakers of English interpret each sentence correctly. A machine using an inadequate model of English would not. It would take "he" to be the subject of the verb + adjective combination, and also of the infinitive; its analysis of sentence 1 would therefore be wrong. A literal interpretation would fail to determine the proper meaning of this sentence and many other sentences. By determining the deep structure the meaning can be more easily arrived at. Alternatively, "easy" may be provided with a feature which would transform "NP be easy to -Inf" to "it be easy to -Inf NP" in accordance with Harris' use of transformations.

Language is structured in this way in all its components, the phonological as well as the syntactic. In both of these components, it is a code, rather than a cipher, system. The human brain knows how to interpret the code. If machines are to interpret language, they must be provided with a comparable capability. Engineers have been working on machine interpretation of the phonological system; it would be useful, for example, if telephone "dialing" could be done by voice, rather than manually. Engineers have not mastered the problem, however, even though they are aware of the basic difficulty.

The problem in the syntactic component of language has been one of the central issues for linguistics since the publication of Saussure's Cours in 1916 (though it was known earlier). Various labels have been given to the underlying structure. Saussure used the traditional philosophical terms: "form" for the underlying structure and "substance" for the surface structure. Recently the term surface structure has been used almost exclusively rather than substance, and deep or underlying structure rather than form.

In view of this structure of language, techniques must be devised to get from the surface structure to the deep structure. Recently linguists have proposed to do so by specifying the relationships between surface and underlying structure by means of transformations. Current linguistic descriptions state these relationships in such a way that the abstract underlying structures are transformed into the surface structures. That is to say, descriptive grammars start from deep structure and relate it to surface structure. If, however, transformations are applied in computer analysis of language, the deep structures must be primarily determined from the surface structures. Accordingly, for the computer, reverse transformations must be devised.

These would not simply be the obverse of transformations linking deep structure to surface structure.

The technique of using reverse transformations was explored almost a decade ago. But it was unsuccessful, primarily because the proposed transformations yielded too many underlying structures, including wrong ones for any given surface structure. In devising reverse transformations, the linguists had not been able to refer to lexical features. The system being developed at the Linguistics Research Center can make use of lexical features, and thus is meeting this problem. Descriptive linguists have also encountered the problem of producing too many alternatives by means of transformations, and are painfully aware of it. Transformations are now generally acknowledged to be too powerful (Bach; Peters and Ritchie, 1971).

This realization has important consequences for machine translation. The fact cannot be escaped that in machine translation, one must somehow determine the underlying forms of sentences. Further, the technique of using some relational formulae like reverse transformations is also clearly necessary. In considering linguistic techniques, the fundamental question is: how can these formulae be adequately restricted so that they yield only the specific underlying structure intended by the author, that is, the proper meaning?

Two devices must be used: the lexical elements must be described as precisely as possible, so that only the desired transformations apply; the transformations must be devised in such a way that their use is properly restricted.

Exploiting this understanding of the necessary procedures will require considerable work. The lexical analysis alone will be a huge task. It will, however, lead to vastly improved general purpose dictionaries, and vastly improved understanding of language, with implications for various applications. Jakobson confirms the inadequate state of dictionaries in all languages Developing the necessary techniques to arrive at underlying structures will also require considerable study.

Like programming techniques, and computers, the linguistic techniques will continue to be improved. Unlike them, the techniques are not yet available for any appreciable amount of any language. As stated above, dictionaries are inadequate. And the fullest account of the grammar of any language: *Integration of Transformational Theories on English Syntax*, by Robert P. Stockwell, Paul Schachter and Barbara Hall Partee, is generally considered premature. For these reasons, specialists state that linguistic analysis is now the major problem if machine translation is to be achieved. The state of linguistics is accordingly of vital importance for machine translation.

4. Contributions of linguistics to machine translation

Two important topics of recent linguistic research have been of major concern for machine translation, and have been in part prompted by work in machine translation: the problem of a universal base, and that of ambiguity in language. Views on each of these topics have changed considerably, even during the last decade. Linguists also differ in their views on each, as the following statement may indicate.

The assumption of a universal base receives support from the capability of speakers to translate. It is also supported by the capability of infants to learn any language, to learn it rapidly, and in accordance with well-determined stages. Whatever a baby's ancestry, it acquires the language it hears. Moreover, the stages of linguistic development are fixed for virtually all infants, regardless of their intelligence.

These observations are most plausibly accounted for if we assume some fundamental principles common to all language; further, that these somehow are related to the functioning of the brain. The principles are highly abstract. They permit certain linguistic structures and constrain others which are theoretically possible. As yet they are not by any means thoroughly explored. The term "universal" has been used for general characteristics of language; one example of a universal may be exemplified here, with two sentences and their variants.

1.a She regretted the fact that she had taken the book.

1.b She regretted the fact that she had taken what?

2.a She regretted that she had taken the book.

2.b She regretted that she had taken what?

2.c What did she regret that she had taken?

BUT NOT 1.c *What did she regret the fact that she had taken?

The impossibility of 1.c results apparently from a universal principle which blocks the extraction of an element out of a clause modifying a noun phrase. This principle was formulated by Ross (1967, 66-70) as "The Complex NP Constraint." Since this principle applies to all languages which have been examined, it is assumed to be a universal characteristic of language.

Whatever the views which will be formulated concerning universals, this principle, like other universal principles that are being investigated, restricts the possible transformations for structures of language. Since language is governed by such constraints, the model which must be constructed to embrace all languages must have certain limits. Moreover, if only because of the finiteness of the human brain, grammars must be finite.

These observations lead to the conclusion that a mechanical translation system can be devised. Even more support is provided by the conclusion of much recent linguistic study that we may posit the existence of a universal base. For the surface structures of any language can be related to such a universal base. Since the universal base in turn can be used for deriving the surface structures of any language, the universal base can serve as the intermediate language between any source language and any target language.

The possibility of devising a translation system in view of the fact that a universal base may exist still leaves many problems. The surface structures of one language may map into the intermediate language differently from those of the target language. For semantic distinctions which are overt in one language may not be overtly expressed in another language. Different languages may also represent differing "word views." We assume, however, that closely related languages, like English and German, are similar in expressing their semantic distinctions overtly and covertly, and even in their surface structures; accordingly, they are relatively easy to translate into each other. Moreover, languages strongly influenced by another language, as were the languages of western Europe by Latin, so that Whorf referred to them as Standard Average European, share many surface features. Accordingly, translation systems may be so designed that the description of any source language is directed at a specific target language. And in our current understanding of language, attempts to move directly from any source language to a universal base, in the hope of translating into any other language, are premature. For we must take into account the complex relationships between a given language and the universal base.

That linguists have not yet been able to determine the exact nature of a universal base does not present a problem for linguists who favor machine translation by means of an intermediary language. Garvin has stated (1970, 9-11) that an intermediary language need be nothing more than a series of symbolic notations to record the output of the recognition routine for the source language and to serve as input into the command routine for the target language.

Further, as mentioned in the preceding chapter, rigorous procedures for establishing relationships between surface structures and a universal base must be established. For example, as Ritchie and Peters (to appear in

Information Sciences) have demonstrated, the transformations characteristic of most generative grammars are so powerful that they permit the derivation of any structure. Consequently, in spite of their contributions to our understanding of language, they can make no claim on properties of grammars, on a universal base, or on what is going on in the brain. Current transformational grammars are accordingly inadequate devices for describing languages and also for use in machine translation systems, for which they produce far too many syntactic interpretations of any given sentence.

The production of devices to map surface structures stringently into underlying structures is one of the most serious concerns of current linguistics. Bach's paper noted above (1971) is an example. A device projected by Stachowitz has been described in RGEMT (1970). It makes use of an underlying form, the standard strings of a language. Associated with these strings are canonical forms, which represent the meanings of given sentences. "The language" of these is assumed to be "common to all natural languages" (Stachowitz, 1970, T-65). Fuller information on the model is given T-66ff. The quotation here may be adequate to indicate that the canonical forms correspond to a universal base. A description of translation as it is being pursued in accordance with this model at the Linguistics Research Center is appended (Stachowitz paper).

Of great importance for the Linguistics Research Center system is a well-designed lexicon. The intensive lexicographical work which has been going on at the Center for more than two years now has resulted in great amounts of syntactic information; the incorporation of semantic information is currently in progress. Because of their syntactic and semantic classification, the lexical entries will limit the possibilities of relationship with canonical forms. In this way a proper match will be brought about between the lexical and syntactic elements of the source language and those of the target language.

The design of the lexicon has been vastly improved over dictionaries envisaged a decade ago. Without intending to dwell on the naivete of these and their proponents, reference might be made one further time to the saying which was supposedly quite ambiguous and accordingly a prime exhibit of the difficulties of machine translation: "Time flies like an arrow." When one notes that Austin's How to Do Things with Words was available at the time this saying was widely discussed, the singular limitations of a view of linguistics which could permit the citing of the proverb seem quite remarkable. For as Austin made clear, meanings cannot be determined from syntax alone. Additional publications, such as Speech Acts by John R. Searle (1969), make the simplistic attention of a decade ago to a highly limited type of syntax quite difficult to understand.

As linguists have improved their models of language, the problem of ambiguity has been reduced. It may be noted that the attention to the pragmatics of Peirce repeats a position held early in machine translation research. By attention to pragmatics, that is, to information on the "origin, uses and effects" of language, sentences belonging to the class of proverbs are not treated like sentences found in scientific exposition. Attention has also been focussed on such classes as illocutionary verbs, or on the characteristics of speech acts. Thus a sentence like: I pronounce you man and wife would not be treated as a simple declarative statement, with the meaning of "pronounce" in a sentence like: They pronounce greasy with a voiced groove fricative.

While models of language in this way incorporate far more information about individual sentences than did the purely syntactic-based grammars of a decade ago, means must be devised to take account of the more accurate analysis of language which is now projected. Suggestions vary concerning the implications of these developments, as the following chapters will indicate. Some specialists consider machine translation unlikely unless at the same time automatic information and fact retrieval are made possible. Others hold that machine translation is not now, and may never be, contemplated for types of language outside technical and scientific documents; accordingly the "origin, uses and effects" of the material to be translated are determined, and investigators dealing with machine translation should direct their concerns at this restricted type of language.

Whatever steps are selected to employ findings of contemporary linguistics to carry out machine translation, it should be noted that specialists in machine translation have taken account of these findings. There accordingly is no conflict between specialists in descriptive linguistics, linguistic theory and machine translation, as Chapter 6 below will outline in further detail. As descriptive linguists improve their understanding of language, and the models by which to express that understanding, machine translation specialists will update their procedures and models.

5. Pertinent recent work in linguistics

Contemporary linguistics is concerned with all facets of language, its syntax, semantics, aberrant uses, uses in established social situations, its relation to other disciplines, such as logic and so on. This breadth of concern contrasts strikingly with self-imposed limitations of the recent past. It also leads one to examine the extent of concern of machine translation with these matters. For machine translation is an application of linguistics, like language teaching, design of communication channels, and the like. Each of these applications relies in part on disciplines other than linguistics. Each may or may not be involved with the various sub-disciplines of linguistics.

To what extent applications are intricately tied to the various concerns of linguistics is a highly important problem; for many of the most active theoreticians in linguistics deplore their inadequacy in understanding language. Some state that an understanding is not foreseeable for a considerable length of time, such as half a millennium. Moreover, there is no comprehensive linguistics theory. Most of the current efforts of linguists, even those who style themselves theoreticians, are directed at minutiae. These efforts, such as the repeated examination of quantifiers like some and any, are carried out chiefly to find support for new hypotheses. If adequate language teaching, or machine translation, or any other application of linguistics is so dependent on a thorough understanding of language that it must be deferred until linguists arrive at a satisfactory linguistic theory, anyone with an interest in these fields should be advised of his dubious expenditure of time, energy and funds.

In dealing with this important question, we may examine the concerns of those linguists who are especially vocal about our lack of understanding of language. John Ross furnished an excellent example of these concerns in one of his presentations sponsored under the Study. It will be summarized briefly here to illustrate topics which engage the interest of linguists, and their possible pertinence for machine translation.

For some time linguists have been concerned with the role of quantifiers in language, such as *some* : *any*. Among such recent linguists are Brugmann, Jespersen, and Sapir. Their discussions have not solved all of the problems with quantifiers. Current concern extends beyond the simple uses, such as those included in Fowler (1965, 31): "*Have you any bananas? No we haven't any bananas*. But *yes we have some bananas*." This quotation implies that *some* is used in positive contexts, *any* in negative, even though it is provided in Fowler's discussion of *any* (= some) in positive sentences like: "*Then, for the first time, she paid any attention to my existence*." While Fowler's entries for *any* and *some* concentrate on stylistic matters, contemporary linguists deal with the syntactic and logical problems. For example, they may note that in addition to other restrictions that cannot be included here *any* is not used before verbs with negative implication, such as *doubt*, *dislike*, and predicate adjectives such as *unkind*. For example, the following sentence is impossible: **Anyone doubts that the earth is flat*. But the following sentence is possible: *Anyone would doubt that the earth is flat*. Similarly, in contrast with the impossible: **Anyone is unkind* the following sentence is possible: *Anyone who fails to send his mother a card on Mother's Day is unkind*. The precise constraints have not yet been defined.

One of the problems in this definition is the existence of two meanings of any, one illustrated in Ross's sentence:

Anybody could have shot Max

The meaning of any here might be made more precise by adding: whatsoever. This use of any is found only when possibility is involved; it is not used with must. A different meaning (some) is found in the question: Do you know any songs? If, however, stress were put on any, especially in a negative sentence, the "any...whatsoever" meaning would emerge.

Accordingly, as study of quantifiers has been pursued more extensively it is clear that a sentence like the following can have two meanings:

We do not believe that any catalyst could have precipitated the reaction

On one interpretation, this sentence could be roughly equivalent to: that some catalyst, that is, that a selected catalyst was involved. By another interpretation it would be equivalent to: that any catalyst whatsoever was involved, that is, that no involvement by a particular catalyst was possible.

This indeterminacy of usage in English presents a translation problem, for German, too, indicates slight differences of meaning with quantifiers such as irgendein 'someone, anyone'. These differences are especially important in the colloquial, as the following quotation from the Duden Grammatik (1959, 265) may indicate:

Irgendeiner muß es doch getan haben!

"Someone must surely have done it!"

Such usages are often found in conjunction with modals and adverbs, like doch in this sentence. For the examples of any which Ross cited, however, translation would not be a problem inasmuch as the ambiguity in English is preserved when literal translation into German is carried out.

The use of any in such a relatively straightforward sentence is simply the beginning of Ross's interest. He has pursued the difference in uses of any in syntactic constructions illustrating general syntactic patterns or principles which he has investigated intensively. One of these is a "maximal domain of syntactic processes,"—in his word "island".

An island is subject to special constraints, as the impossibility of the following example indicates:

*Who do you believe their claim that Max fired?

If "their claim" were eliminated, the equivalent sentence is possible:

Who do you believe that Max fired?

By the use of claim, an island is created which does not permit the extraction of the object of fired. See Chapter 4 above.

In his discussion, Ross proposed that islands have an effect on the meaning of any, e.g.

We do not believe their claim that any catalyst could have precipitated the reaction

For Ross, any in such a context can only mean "any...whatsoever". Not all participants in the Conference agreed. Yet the situation is presented here nonetheless to illustrate the delicacy of concern which occupies current syntacticians. Readers may decide for themselves the importance of possible ambiguities in such sentences if machine translation were confronted with them.

Another syntactic principle explored by Ross is in his words the domino effect. By this principle, selected elements like *deny* affect subsequent elements in the sentence. Thus, *deny* rules out the last of the following sentences even though the others are possible:

1. I deny that anybody said anything nasty
2. I deny that anybody said something nasty
3. I deny that somebody said something nasty
4. *I deny that somebody said anything nasty

In the same way, according to Ross, *any* cannot be used in lower clauses when *some* is used in upper clauses, as in:

Finding somebody under the bed is not easy for some people

That is, if the first phrase read: *finding anybody under the bed*, the sentence would be impossible, on Ross's view. On the basis of such patterns, Ross asserts that domino constructions disambiguate. Accordingly, the following sentence can only have the "any...whatsoever" interpretation:

We did not inform some researchers that any catalyst could have precipitated the reaction

From these examples, Ross concludes that in machine translation the sense of *any* would not be determined until the machine had searched for *some* in the same sentence. That is, machine translation would be faced with a large job. For the meaning of individual words could not be determined simply by examining the immediate contexts. Rather, entire passages would have to be examined. The problem would be compounded because linguists have not yet determined the extent to which individual areas contribute to disambiguating meaning; see Chapter 6.

Ross cites these and other problems in support of his contention that linguists have a highly inadequate view of language. The extent of his pessimism may be illustrated by his statement that the sentence: Birds sing is full of a host of unsolved problems.

As noted above, individuals will have to evaluate our current understanding of language and determine for themselves whether Ross's bleak characterization of the state of linguistics implies that machine translation now is impossible. The extent to which machine translation depends on the production of a comprehensive linguistic theory was discussed repeatedly during the Study. Obviously the sentence: Birds sing can be readily translated: Vögel singen, whether or not Ross's problems are solved. In much the same way, many complex constructions may be translated straightforwardly, especially between closely related languages.

It may not be presumptuous then to suggest on the basis of Ross's problem-riddled example that man and machine can happily embark on translation projects, even before current and future generations of linguists solve its momentous problems.

For students of translation, a second paper may furnish more comfort concerning the contributions of linguistics--the paper of Fillmore which undertook to determine the possibility of finding an unambiguous interpretation for the sentence: May we come in? Fillmore selected the sentence, partly because it is extremely barren in providing assistance to a hopeful interpreter. It contains no content word for the subject, as does the sentence: Birds sing, to delimit the meaning of actor. For like other auxiliaries, may has a broad scope of meaning. Yet Fillmore demonstrated that proper use of all linguistics features leaves little question about the proper meaning of the sentence.

Since the paper is appended, it may be consulted for details. The general types of analytic techniques, as well as the types of "linguistic information" in this sentence pointed out by Fillmore will be summarized here. Moreover, in evaluating the implications of Fillmore's analysis for machine translation, it is important to note that he used only the written form of the sentence, excluding information that might be obtained from "any understanding of the voice quality of the speaker on the manner of utterance."

Identifying first the "syntactic information" in the sentence, Fillmore uses it to determine among the three possible functions of may the one which is appropriate in this sentence.

Next, examining the "illocutionary force of the question," Fillmore notes the information on deixis furnished by the pronoun and the verb come. The term "illocutionary force" refers in Fillmore's paper to the obligation which the question imposes on the addressee, that is, the obligation to exercise authority. The term "deixis" refers to the various aspects of the interpretation of sentences that relate to the speech act situation, such as person deixis, place deixis and time deixis. The possible meanings of come are restricted by its use in "a permission-seeking utterance."

Last, it may be noted that Fillmore determines the meaning of the sentence from its "surface structure." He has done so by using a comprehensive lexical description for each of the four words. The possible meanings of each are restricted by the order of the sentence and by the selection of the other elements. That is to say, disambiguation was carried out by using two syntactic devices: order and selection.

In conclusion, Fillmore lists "the various kinds of facts which must... be included in a fully developed system of linguistic description." These are extensive. Yet such explicit linguistic descriptions permit a mechanical disambiguation, and interpretation, of a given sentence.

The effort required to produce these descriptions will, however, be enormous. An example of the analysis necessary for improved interpretation of sentences, which will be particularly important for information processing, is Karttunen's paper, "The Logic of English Predicate Complement Constructions." This paper, which is also appended, leads to seven classes of verbs, each indicating a commitment which "the main sentence carries along...with respect to the truth or falsity of its complement" and an indication of "what is implied." For example, the verb cause belongs to one of these classes which carries a commitment "true" for main as well as complement sentences. The seven classes of verbs arrived at in the paper identify meanings in much the same way as did the syntactic information in the sentence: May we come in?

Linguists accordingly are drawing nearer to lexicographical work of the past, as represented especially by Zgusta and Josselson in the Study. Since the use of lexicographical techniques for machine translation is discussed in the appended papers of Zgusta, they will not be further noted here.

Current linguistic description in this way is providing information on detailed lexical classes, as well as on syntactic constructions. These two types of information about language, whether they be labeled syntactic or semantic, are leading to descriptions of language which are so precise that the sense of a sentence can be determined mechanically.

The generative semanticists, besides Ross, who participated in the Study: George Lakoff, Robin Lakoff and James McCawley, are also contributing insights which will sharpen our understanding of language. McCawley, for example, challenged the analysis of adjectives as reduced forms of relative clauses. In his presentation he cited examples, such as: *He's an incredible fool* which cannot be derived from: He's a fool who is incredible. Further, while the sentence *John is an easy man to please* is acceptable, the construction is inacceptable for a noun like that in the sentence: **John is an easy tool-and-die-maker to please*. Such observations point out the necessity of distinguishing sub-classes of "adjectives" and carefully defining their uses in somewhat the same way as Karttunen and other linguists are distinguishing sub-classes of verbs.

These linguistic analyses will not however solve the problem of sentences which Austin labeled "performatives". One of the participants in the Study, Fraser, concerned himself particularly with the problems raised by Austin, and pursued by other philosophers such as Searle. As Austin pointed out, performative sentences contain "humdrum verbs in the first person singular present indicative active." An example is: "I name this ship the Queen Elizabeth." Few linguists are unaware of Austin's book *How to Do Things with Words* (1962). It is noteworthy that the book concludes with a sub-classification of the verbs discussed. There are five classes:

1) verdictives, 2) exercitives, 3) commissives, 4) behabitives and 5) expositives. The example given above belongs to Austin's class 2. Like the work of the generative semanticists, Austin's leads to more precise lexical analysis.

Modestly, Austin does not claim to have produced a definitive classification, but includes in his final chapter the statement: (1962/68, 148) "Now we said that there was one further thing obviously requiring to be done, which is a matter of prolonged fieldwork." Generally, fieldwork is used by linguists of work in non-literate cultures. Austin obviously recommends highly exacting analysis of the types discussed earlier in this chapter.

After Fraser's presentation, the problems raised for machine translation by illocutionary verbs were discussed, especially by Ross and Bar-Hillel. Like Fraser, Ross stated that a theory of speech acts is not essential for machine translation, inasmuch as its goal is the translation of technical materials. Agreeing with this view, Bar-Hillel added that nonetheless we need a great deal of pragmatic information.

This chapter was intended as a survey rather than as an evaluation of current linguistic work as pertaining to machine translation. Linguists are carrying on research into the narrower concerns of language, and those listed in the first paragraph of this chapter. Though not all of this work may be directly pertinent for machine translation, specialists in machine translation are profiting greatly from it. The implicit interest of this work--to analyze sentences so thoroughly that they can be interpreted from the linguistic information contained within them--is of great significance for machine translation.

To the extent that this interest is accomplished, machines will be able to translate.

6. Views of specialists concerning machine translation

One of the primary problems in presenting the views of specialists in machine translation results from the low level of research during the past five years. Few groups received any kind of support. The greater part of them could only update their previous systems, not introduce major innovations. In view of the low funding, research was severely restricted, generally devoted to improvements in the lexicon. This limitation in funding greatly restricted the possibility of carrying out new experiments, let alone that of producing improved translation systems which could meet some of the goals held out for machine translation. The views of specialists are accordingly based in part on assumptions framed some years ago when some long-range machine translation projects were able to carry out work in programming and in linguistic analysis, and to test their efforts by means of computer runs.

In his summary on the final day of the January Conference, Bar-Hillel concentrated on the linguistic situation. Noting that the primary considerations are quality, speed and cost, he expected improvements in speed and cost of output from advances in computer hardware and software; but their contributions to improved quality would only be external, for example as printouts would begin to approximate those produced by printing-presses. Essential for improving quality is improvement in linguistic theory and analysis.

Bar-Hillel's discussion involved arguments on a definition of quality, and on the receptivity of scientists to output from the translation systems which now are in use, notably the Georgetown system as used at Oak Ridge. This point will be discussed further below, in connection with Zarechnak's statements.

Bar-Hillel disagreed with Zarechnak's statement that the output was approximately 80% complete. His own estimate is around 35 to 40%. He assumes that sustained linguistic work during the next five years can raise this figure by 15%. And he considers this degree of accuracy less than that which sponsors demand. But as a perceptive participant at the Conference pointed out, Bar-Hillel's estimate has to do with readability. Zarechnak's on the other hand has to do with informativeness, as the reaction of the Oak Ridge scientists indicates.

Turning from this pessimistic estimate to possible palliatives, Bar-Hillel rejected the possibility of simplification of scientific texts by editors, and he also doubted that scientific texts presented many fewer problems than do general materials. Among the questions discussed during Bar-Hillel's summary were inadequate means devised to measure complexity. Fraser pointed out the difficulties involved in determining such means, for complexities may exist at the various levels of language, by no means in parallel ways. The question accordingly is another which has been raised in machine translation research.

In what he considers the present impasse, Bar-Hillel proposed that fully automatic machine translation must be sacrificed for the time being, and that instead efforts should be made to develop man-machine combinations for translation. He had no suggestion on the type of combination, but indicated that the most promising one would have to be determined by research. During the discussion Pankowicz pointed out that no translation, indeed no writing, is published without editorial intervention. Bar-Hillel envisages more "human intervention" than is the normal practice in translation. He concluded by suggesting that determining a useful type of man-machine translation system would not be difficult.

However negative he is to fully automatic machine translation at present, Bar-Hillel recommends that further research be carried out. In his view, such work is intellectually respectable and challenging. It is of further interest to him because of its potential contributions to information retrieval.

Garvin is far less pessimistic about the prospects for machine translation. In his view, the basic problems involving linguistic analysis have been identified. What remains to be done is application, involving detailed lexical analysis. By Garvin's view, if adequate funding were provided, acceptable outputs of machine translation could be achieved in five to ten years.

Neither Garvin, nor any of the other participants, however, were able to define what is meant by acceptable translation. Moreover, Garvin believes that linguists must leave such determination to users. A similar position was maintained by Kay, who stated that "buyers" of translation are the persons best equipped to pass judgment on the acceptability of translations. A comprehensive judgment accordingly is unlikely until machine translation is carried out.

Garvin's views on the essential problems are indicated in his appended paper. For him too, the linguistic problem is primary. Moreover, a system must be prepared in accordance with an adequate model. He states further that all systems under development are of a tripartite design, but that grammar and computer programs may be linked. Specific problems, in his opinion, must be solved in terms of the overall system. There can be no hard and fast rules, for example, for deciding whether to handle any particular topic in the grammar code or in the algorithmic portion of the system. Garvin is also flexible in his recommendations for performing analysis; he does not advocate rigid analysis from left to right, but rather selective analysis. As an experienced practitioner in the field, accordingly, Garvin believes in making use of any advantages which can be offered by linguist or programmer. The designer of a translation system relies on them for any possible assistance, but like any applied scientist he must decide when their contributions are useful in helping attain his ends.

Of particular interest is the IBM question answering system mentioned in Petrick's appended paper. This system is based on a generative transformational syntactic component that reflects current transformational theory to a reasonable extent. Deep structures are in some cases quite deep; certain nouns for example, are transformationally derived from underlying abstract verbs. The use of relatively deep structure facilitates semantic interpretation; this is accomplished through the use of a translation mechanism due to Knuth. (The task of relating these deep structures to surface forms is, to be sure, quite complex. Even relatively simple sentences may require as many as forty or fifty transformational applications.)

The syntactic analysis algorithm which is utilized is valid for a significant class of transformational grammars. This, together with the modular nature of the Knuth semantic interpreter, makes modification of both the syntactic and semantic components relatively easy.

It should be noted that the system being implemented at the IBM

Research Center is an experimental question answering system based on a restricted subset of natural English, not a machine translation system involving relatively unrestricted textual material in two or more natural languages. This choice of application was made because the Theoretical and Computational Linguistics Group at the IBM Research Center feels that the coverage of English presently attainable by any means is too small for the purposes of machine translation but perhaps not too small for applications which can tolerate artificially restricted input. It should also be noted that the IBM system is in an early state of development with the separate components (generative transformational grammar, syntactic analyzer, and semantic interpreter) yet to be integrated and put to use as a coherent whole.

By contrast, the system used for machine translation of Russian at Oak Ridge is essentially the GAT system developed at Georgetown University some years ago. The lexicon and syntactic rules are updated by Zarechnak constantly. But the basic system, based on a surface structure analysis, has been maintained. As indicated above, the system is frequently used. According to report, the output is valuable, though scientists must become used to its Russian-like syntax. If translation indicates that a paper is particularly valuable, a further effort can be made to produce a completely accurate version of the original. Zarechnak considers the output of great value to the Oak Ridge scientists. They must also find it useful, for numerous translations are produced on the initiative of individual scientists.

As several participants in the Study pointed out, a convincing analysis of the usefulness of machine translation will result only from a well-designed experiment determining reactions to its output. Yet linguists have identified the stages of accuracy, in accordance with the levels of language which must be incorporated in a system, as shown in the examples below. It is unlikely that scientific or technical articles would contain such simple sentences. Yet on the assumption that no further linguistic information were available in the sentences to be translated, these examples illustrate successive degrees of sophistication in the development of machine translation and the expected quality at each stage.

Proceeding from the simplest system to one which approximates the information available to a human translator, we may propose the following progression:

translation with: 1. Lexical information alone

2. Syntactic information
3. Semantic information
4. Contextual information
5. Pragmatic information

Each state incorporates all of the earlier stages.

The following examples indicate difficulties, and characterize inadequacies, which each type of system fails to resolve. If that type of system were used, these shortcomings would have to be removed by pre-editing or post-editing.

1. Lexical translation, with no access to syntactic information.

They milk cows.

Under such a system milk might be taken as verb or noun.

2. Syntactic translation, with no access to semantic information.

The conductor broke.

The conductor smiled.

Under such a system disambiguation would be impossible.

3. Semantic translation, without contextual theory.

We watched the conductor. He smiled.

We watched the conductor. It was on fire.

Here too diambiguation would be impossible.

4. A system making use of contextual information would be able to disambiguate the sentences given in 3, and other examples, such as the German verb beugen in the indicated contexts.

beugen = bend
= deflect (optics)
= inflect or conjugate (grammar)

5. A system with access to pragmatic information would provide for the German sentence "Eisenhower folgte Truman" the correct reading "Eisenhower succeeded Truman" rather than the equally correct alternate translation "Eisenhower obeyed Truman."

In accordance with this sketch of potential systems, we expect the highest quality from a system which is at stage 5, or possibly at stage 4. The requirements for these stages have not yet been handled in linguistic theory, and accordingly at present they are unattainable. To what extent a system at stage 3 will be able to translate scientific and technical materials acceptably will depend on testing of the output, and the receptivity of users after such a system has been developed. Systems at this stage are now under development.

The questions raised in the Study are also of interest to scholars who could not participate, as a recent article by Kulagina, Mel'chuk and Rozentsveyg indicates. It is noteworthy that, like Bar-Hillel and other participants in the Study, the three authors concentrate on the quality to be achieved, assuming that cost and time can be adequately managed.

The authors express their views concerning the feasibility of machine translation with regard to the ALPAC report, especially its view that machine translation is at present impractical. They state: "We wish to declare decisively that this view has no real support: it is founded upon a failure to understand the problem in principle and confusion of its theoretical, scientific and practical aspects. The fact that machine translation has been ineffectual in practice to the present should, in our opinion, lead to an increase rather than a decrease in efforts in this area, especially in exploratory and experimental work. It is clear that no practical result can precede fundamental development of the problem, although the possibility is not excluded that useful practical results may be the product of early stages of research. There is not, and has not been, a crisis in machine translation as a scientific undertaking, a crisis which would be reflected in a lack of ideas and a lack of understanding what path to follow. Machine translation as a scientific undertaking...is continuing to develop actively. There are many interesting ideas and approaches which are far from being sufficiently developed and experimentally tested."

After making this critique of a negative approach, they state that a high-capacity, high-quality system can be established within about five years if (a) the components produced under a system--dictionaries, grammars, algorithms--are prepared on the basis of available theoretical developments and experimental results, (b) goal-directed experiments are carried out (given an existing algorithm), and (c) research oriented toward a future modification of the system is carried out.

Having arrived at such a judgment, the authors suggest the establishment of an MT center comprised of a linguistic, a mathematical and a computer group equipped with the best computational equipment available. From such a center they expect not only contributions to machine translation, but also to automatic language processing in general. They also believe that if their recommendations are followed, machine translation on an operational scale can indeed be accomplished within their estimated deadline (+ five years).

The estimates, and recommendations, of this article coincide remarkably with those of some of the participants in the Study, notably Garvin. Readers may form their own judgments on the basis of the appended articles, not all of which agree in these estimates or recommendations. Like Garvin, Kulagina, Mel'chuk and Rozentsveyg have been involved in the theoretical and practical aspects of machine translation. Their views are accordingly based on intimate knowledge of the difficulties involved in attempting to achieve high quality machine translation.

Conclusions

1. A technological application drawing on three bases: computer hardware, computer software and linguistic analysis, machine translation today is confronted with fundamental obstacles only in the last. Advances in computer hardward and software have greatly reduced the earlier problems in these areas.

In spite of the progress that has been made in linguistic analysis, linguistic research has dealt primarily with syntactic analysis of individual sentences, and hardly at all with semantic problems and discourse analysis. As a result, current linguistic theory is inadequate for machine translation. For machine translation, semantic representations derived from syntactic structures in the source language must be associated with syntactic structures in the target language. See Katz-Postal, An Integrated Theory of Linguistic Descriptions, 1964, 166-172. To meet this problem, linguists must concern themselves with performance models and with semantic and discourse analysis. Moreover, comprehensive grammars do not yet exist for any language. Estimates of the availability of adequate grammars vary. Production of such grammars depends on the complexity of the model of language and on the research support provided. Some scholars participating in the Study suggest a date of five to ten years, a figure proposed also in a study carried out by Soviet specialists.

In view of the Peters-Ritchie results, it may be advisable to continue efforts with more restricted grammatical models which provide exact surface analysis based on syntactic and semantic features in the lexicon. Examples are string analysis, the model used at the Linguistics Research Center, dependency theory of the Soviet type, and grammatical models whose transformational apparatus is more restricted than that of "standard" transformational grammars, for example, systems which use non-ordered or partially ordered transformations or equivalence transformations. Further, research in discourse analysis should be increased. Since the problems in machine translation are not the generation of coherent discourse but the carrying across of information, the achievement of translation would be considerably facilitated by such models. These problems may even be less pressing in actual practice because of the user reaction; that is,very often it may not be necessary for the system to represent all alternatives since the user will be able to provide the proper reading because of his access to information necessary for comprehension. Investigations on user-translation interaction should be carried out, especially in view of the highly divergent estimates of Zarechnak and Bar-Hillel. See also section 4, p. 46.

2. Like other technological applications, machine translation can be designed with various degrees of adequacy. The history of machine translation reflects this situation. The first attempts were primarily lexical. Syntactic analysis was then added. Currently semantic analysis is included for projected machine translation systems.

The improved understanding of language resulting from these progressively more comprehensive descriptions of language leads to improved translations. Translations based on semantic analysis will be correct when the information needed for disambiguation of a sentence is contained in that sentence. When it is not, contextual and pragmatic information will be necessary.

3. Meaning is largely determined by the semantic readings of the lexical items in a sentence and the syntactic (semantic) relations between those items; these are presumably represented by the underlying structures of language. To arrive at the meanings of specific sentences, the underlying structure will have to be determined from the surface structure. In related languages, such as English and German, the relationships between surface and underlying structure are more similar than they are between less related languages like Russian and English or unrelated languages, such as English and Chinese. Accordingly, it will be simpler to devise translation systems for related languages. For the development of the technology of machine translation, systems designed for related languages are accordingly recommended at this time as an immediate goal. Medium-range goals (Russian-English) and long-range goals (Chinese-English) should also be planned.

4. The usefulness of translation depends on various factors: cost, timeliness, comprehensibility. In locations where imperfect, lexically-based machine translations are available, scientists have selected these over human translation when they could be made available the following day and human translations only after a week. In view of this situation, studies should be performed to measure the extent to which comprehensibility of a translation is dependent on the knowledge available to the actual user. Moreover, it should be noted that timeliness ranks high as a factor in translation. See also page 46.

Participants in the Study did not agree on what constitutes "high quality" translation. There is apparently no absolute standard. Rather, standards must be defined with reference to specific users and specific purposes.

Recommendations

1. On the basis of this Study it is recommended that support be made available for research in machine translation. The recommendation is made on the grounds that quality translation can be achieved in the near future. This recommendation agrees strikingly with conclusions reached in a study carried out in the Soviet Union.

Moreover, apart from attempts in information retrieval, machine translation is currently the only discipline which requires the study of problems beyond the sentence boundary. Because of the general lack of interest in these problems on the part of linguists, machine translation should be sponsored as an intellectual pursuit contributing to our knowledge of language.

2. For improved machine translation, research in the areas of descriptive linguistics, theoretical linguistics, comparative linguistics, stylistics, and evaluation of translation is necessary and should be supported.

2.1 Lexical research is necessary to determine the syntactic and semantic patterns of linguistic entities. Recent lexical research has indicated that entities such as verbs which have more than one meaning may have a particular meaning (1) only when they occur in specific syntactic environments whereas they have meaning (2) or further meanings when they occur in other specific environments. To illustrate the effect of only a trivially improved lexicon on translation, the report of an experiment conducted by Stachowitz in the spring of 1967 is appended.

2.2 Continued syntactic research, based on comprehensive lexical research, is essential. Fortunately a great deal of such research is being carried out, though eclectically, by linguists and their students in the normal course of their activities. Funding of such research should be increased, as well as linguistic study carried out in accordance with various approaches to language.

3. Theoretical research is essential, especially in view of the conclusions arrived at by Peters and Ritchie with reference to current transformational grammar. Various models based on differing grammatical assumption and/or using less powerful transformations should be investigated. Among thes are the string analysis of Harris, and models like those of the Russian linguists which are based on dependency grammars. See Conclusions, p. 44.

Besides encouraging research in discourse analysis and production of coherent discourse, the possibility of establishing a research area intermediate between sentence (constituent) analysis and contextual analysis should be investigated. This investigation would be concerned with notions like those proposed by Fraser on "usage of sentences."

4. Contrastive studies dealing with the lexical and syntactic structures of two languages, and with the similarities of mapping these structures into semantic representations should be carried out. Such studies should also be concerned with the ways in which covert and overt semantic distinctions are expressed in these languages, including their overlap.

The overlap of lexical, syntactic and semantic ambiguity between two languages should be studied from the point of view of carrying source language ambiguity over into the target language.

5. Descriptive stylistic studies on the incidence of lexical, syntactic and semantic ambiguities in scientific texts and their resolution by means of sentence immanent, context immanent and context external information should be carried out, as well as contrastive stylistic studies on scientific texts in contrast with literary texts. Such studies should aim to determine the types of syntactic structures used in various "styles" of language and possible divergences between them.

6. Explicit study should also be made of the kind of information available to the user which is necessary for the understanding of material that is mechanically translated. Such studies should seek to determine the amount of knowledge available from the surrounding text, as well as the amount of world knowledge necessary for the understanding of individual sentences. These investigations would be designed to determine the amount of information which must be provided to the machine so that the output is intelligible to a specialist or a general user.

7. Since the results of linguistic research will contribute to advances in machine translation, support is also recommended for research on problems in linguistics.

APPENDIX I

Conferences at the Linguistics Research Center

June 15 - 19, 1970

Martin Kay, W. P. Lehmann, Norman M. Martin, Jacob Mey, Stanley R. Petrick, Robert F. Simmons, Rolf Stachowitz, Donald E. Walker, Ladislav Zgusta, and the Linguistics Research Center staff.

September 30 - October 2, 1970

Emmon W. Bach, Charles Fillmore, Lauri J. Karttunen, W. P. Lehmann, John Lyons, Norman M. Martin, Jacob Mey, P. Stanley Peters, Robert F. Simmons, Rolf Stachowitz, and the Linguistics Research Center staff.

December 16 - 18, 1970

Emmon W. Bach, Lauri J. Karttunen, Robin and George Lakoff, W. P. Lehmann, James D. McCawley, Norman M. Martin, P. Stanley Peters, John R. Ross, Robert S. Simmons, Rolf Stachowitz, and the Linguistics Research Center staff.

January 11 - 15, 1971

Emmon W. Bach, Yehoshua Bar-Hillel, Paul L. Garvin, J. Bruce Fraser, Lauri J. Karttunen, Martin Kay, W. P. Lehmann, John Lyons, R. Ross Macdonald, Norman M. Martin, Z. L. Pankowicz, Eugene D. Pendergraft, P. Stanley Peters, Stanley R. Petrick, John R. Ross, Robert F. Simmons, Rolf Stachowitz, Rowena Swanson, Donald E. Walker, Terry A. Winograd, Michael Zarechnak, Ladislav Zgusta, and the Linguistics Research Center staff.

Visits of Individual Consultants

March - April 1970

Yehoshua Bar-Hillel

April 1970

Roman Jakobson

July 1970

J. Bruce Fraser

September 1970

Paul L. Garvin

February 1971

Harry H. Josselson

March 1971

Roman Jakobson

APPENDIX II
Papers of participants

Supporting papers. As the statement on work under the Contract indicates, participants in the Study presented papers or participated in informal discussions. Some of the presentations reflect views of the authors which are published elsewhere. Participants were not pressed to provide papers for the final report. Further, the views which are expressed in the accompanying papers are strictly those of each author. Some of the authors have modified their statements after taking part in one or more conferences. But each paper included here was provided by its author and is given without modifications in the form provided originally by its author.

Most readers of the Report are probably acquainted with the authors of the appended papers. It has seemed unnecessary to provide introductions for such outstanding figures in linguistics and related disciplines. The appended bibliography, which is highly selective, will provide further access to the authors and their views.

Titles of Papers in Appendix I

Y. Bar-Hillel:	Some Reflections on the Present Outlook for High-Quality Machine Translation
C. F. Fillmore:	On a Fully Developed System of Linguistic Description
P. L Garvin:	Operational Problems of Machine Translation: A Position Paper
L. Karttunen:	The Logic of English Predicate Complement Constructions
J. Lyons:	The Feasibility of High Quality Machine Translation
N. Martin:	Philosophy of Language and the Feasibility of MT: A Position Paper
J. Mey	Toward a Theory of Computational Linguistics
E. Pendergraft:	Meaning Revisited
S. Petrick:	Syntactic Analysis for Transformational Grammar
________	Syntactic Analysis Requirements of Machine Translation
A. Stachowitz:	Analysis of Es liegt eine grosse Anzahl von Elementen vor
R. Stachowitz:	Lexical Features in Translation and Paraphrasing : an Experiment
________	Requirements for Machine Translation: Problems, Solutions, Prospects
D. Walker	The Current Status of Computer Hardware and Software as it Affects the Development of High Quality Machine Translation
L. Zgusta:	Equivalents and Explanations in Bilingual Dictionaries
________	The Shape of the Dictionary for Mechanical Translation Purposes

THEORETICAL STUDY EFFORT OF HIGH QUALITY TRANSLATION

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SOME REFLECTIONS ON THE PRESENT OUTLOOK FOR HIGH-QUALITY MACHINE TRANSLATION

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I shall take off from Josselson's recent report[1] on the progress of MT research in the 60's. By starting there, I shall be able to restrict myself to pointing out certain issues that seem to me not to have been adequately treated in the MT literature, including the most recent one.

For high-quality MT it is now probably generally recognized that reliance on the best available linguistic theories is a necessary but by no means sufficient condition. But for reasons I have discussed elsewhere at length, even the best modern linguistic theories do not treat adequately the pragmatical aspects of communication in natural languages. As a matter of fact, these aspects are simply not yet sufficiently well understood to receive a satisfactory explicit formulation, not even to the degree that the so-called semantic aspects have already received.

Applied linguistics, or the theory of linguistic performance, of which the theory of translation is a part, works with utterances and sequences of such as basic units. What a translation program is meant to provide a translation for is primarily utterances (written, printed or spoken), and only secondarily, if at all, sentences (or sequences of such); though, in some sense, translation of utterances may still be based on translation of sentences.

It is now almost generally agreed upon that high-quality MT is possible only when the text to be translated has been understood, in an appropriate sense, by the translating mechanism. A full understanding requires taking into account the pragmatic aspects of the text, such as by whom the text was produced, for which kind of audience it was meant, which kind of background knowledge the producer of the text assumed to be available to the audience, the time, the place, and other parameters of the situation in which the text was produced, etc.

In view of the present non-existence of an adequate theory of the pragmatics of communication in natural languages and the small likelihood that such a theory will be produced in the near future, the question arises to what degree disregarding the pragmatic aspects of the production of a given text will diminish the quality of its translation. There exists a good amount of published material on this matter, with regard to human translation, but so far we have little systematic insight and still less experimental evidence.

In principle, understanding a given text means, for instance, being able to tell which, if any, statements the producer of this text intended to make, which, if any, questions he intended to ask, which, if any, commands he intended to give, etc. In order to manipulate these statements, questions, commands, etc., e.g. in order to produce another text in a different language, by which the same statements can be made, the same questions asked, etc., it seems, in general, to be necessary to reformulate these statements, questions, commands, etc., in some constructed, non-pragmatic language, the so-called intermediate or pivot language. This issue has been adequately covered in the literature, and I have no new comments to make, except to state that in view of the enormous difficulties that stand in the way of such a language, any attempt for MT research to wait for the completion of this task would just mean the end of this research.

I am in no position to estimate to what degree a lowering of this aim would enable MT research to continue; a clarification of this issue would be vital. It is not inconceivable that a certain amount of experimentation could be performed with human translators who would be instructed to deliberately disregard some or all of the pragmatic aspects they would usually take into consideration and to compare these outputs as to their degree of satisfactoriness for a variety of users in a variety of conditions.

In other words, it now seems more than ever that the ideal of obtaining MT of a general quality comparable to that of a good human translator has led to a dead end. It seems therefore to be mandatory to investigate, more thoroughly than before, the various possible substitutes: (1) Machine-aided human translation, (2) man-aided machine translation, (3) low-quality autonomous machine translation. These are all eminently practical matters that still have to be based, in some form, on linguistic theory, particularly on psycholinguistics, but will have to rely to a much higher degree on investigations of man-machine interactive systems and on the psychology of the behavior of humans in such conditions.

It seems, then, that we have turned full circle in MT research and are now approximately back where we started some 19 years ago. MT will probably have to rely on language-dependent strategies rather than on some highly developed theory, but it is quite clear that the detour has enormously helped clarify the issue, has dispelled any utopian hopes so that we are now in a much better position to attack this problem afresh.

Let me make a side remark. When I started using the term "high quality" a number of years ago, I was using it in a much too absolute sense which cannot be seriously justified. "High quality" has to be relativized with regard to users and with regard to situations. A translation which is of good quality for a certain user in a certain situation might be of lesser quality for the same user in a different situation or for a different user, whether in the same or in a different situation. What is satisfactory for one need not be satisfactory for another. This remark, of course, is trivial as such, but its implications for MT might not have been always fully realized. It is therefore, for instance, not inconceivable that a translation program with an unsatisfactory output for a certain user under given conditions might turn out to be more satisfactory if the conditions are changed, for instance, if the user is allowed to ask back certain questions and the computer is programmed to answer these questions upon request. Again, clearly, much experimentation is needed to get more light on this issue.

Altogether, I am rather doubtful whether at this stage any further research on the possibilities of normalization, canonization, or other types of regimentation of the input to MT will prove to be useful. At any rate, this type of research, if at all, should not be undertaken by MT research groups but by linguists at large. The results, if any, of their research will have a much broader application than to the problem of MT alone. MT research should restrict itself, in my opinion, to the development of language-dependent strategies and follow the general linguistic research only to such a degree as is necessary without losing oneself in utopian hopes. Every program for machine translation should be immediately tested as to its effects on the human user. He is the first and final judge, and it is he who will have to tell whether he is ready to trade quality for speed, and to what degree.

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[1] Josselson, Harry H. 'A Linguistic Interpretation of MT in the Sixties.' Eleventh Annual Progress Report, Research in Computer-Aided Translation, Wayne State University, August 31, 1969, pp. 1-72.

ON A FULLY DEVELOPED SYSTEM OF LINGUISTIC DESCRIPTION

by

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In this paper I offer a preview of the scope of linguistic description, insofar as the field of linguistics touches on questions of the meanings of sentences. I take the subject matter of linguistics, in its grammatical, semantic, and pragmatic subdivisions, to include the full catalogue of knowledge which the speakers of a language can be said to possess about the structure and structural affinities of the sentences in their language, and their knowledge about the appropriate use of these sentences. I take the special explanatory task of linguistics to be that of discovering the principles which underlie such knowledge. I will exhibit the range of information which speakers possess about the sentences in their language by examining, as thoroughly as I know how, one English sentence. The sentence I have chosen for this demonstration is simple and short and extremely easy to understand; it is the four-syllable question, MAY WE COME IN?

Instead of beginning with a set of observations about a particular utterance of our sentence, by a particular speaker, on a particular occasion, examining all of the social and psychological and acoustic and biological and physical aspects of the situation to which we can gain access, and speculating about which of these can be said to exemplify specifically *linguistic* facts about the sentence in question, I will conduct a thought-experiment that will be simpler and more limited and that will be guaranteed to point solely to linguistic information. The thought-experiment I have in mind is this: we are to assume that we know, about some real-world situation, only one single fact, and that is, that somebody uttered the sentence MAY WE COME IN? A qualification on the nature of that information is that we have it in a notation which fails to include any understanding of the voice quality of the speaker or the manner of utterance. We know only that it was an utterance of the English sentence that we ordinarily have in mind when we write, MAY WE COME IN?

Our task is to make explicit everything that we know about the sentence as a linguistic object, and everything that we know, as speakers of English, about the situation, or class of possible situations, in which it was uttered. We will be interested, in short, in the grammatical form of the sentence, the meanings and grammatical properties of its words, and in the assumptions we find ourselves making about the speaker of the sentence and about the setting in which it was uttered.

Our account will exclude information about the range of possible phonetic realizations of the sentence, and we will furthermore disregard the uninteresting possibility that the sentence was uttered hypotactically: we will not bother with the infinite range of possible conditions for the utterance of this sentence that includes somebody responding to the request that he pronounce four English monosyllables, or a speaker of a foreign language imitating an English utterance he once overheard, or a librarian reading aloud the title of a short story.

Assuming that the sentence was uttered in conformity with the system of linguistic conventions whose character we are trying to make explicit, we will probably find ourselves imagining a situation involving: some kind of enclosure, call it E; at least three beings, call them A, B, and C; one of whom, A, is a speaker of English and is the utterer of our sentence; one of whom, B, is believed by A to be a speaker of English and who is the addressee of our sentence; and some other being, C, who is a companion of A. (I say "being" rather than "person", since C might be, for example, A's pet beaver.) We further assume that A believes that he and C are outside the enclosure E, that A believes B to be inside E, that A is interested in the possibility of his gaining admission to E, in C's company, and that A believes that B has the authority--or represents somebody who has the authority--to decide whether or not A and C may enter E. We further understand that the uttering of this sentence is an act which socially requires B to do something--in particular, to say something--it being understood that what he says will count as authorizing or forbidding the move into E on the part of A and his companion C.

These, then, are the main things that we might find ourselves imagining, on learning about a particular situation that somebody has made the request, MAY WE COME IN?. This is the most straightforward understanding we might have of the appropriateness conditions for uttering our sentence. Actual situations in which the utterance is used may depart from this description in several ways and for several reasons: some of A's beliefs may be mistaken; A may be speaking insincerely; the number of A's addressees may be greater than one; and the number of A's companions may be greater than one; and other arrangements of the personnel are conceivable.

As linguists we need to ask what it is about the structure of the sentence MAY WE COME IN? that makes it possible for speakers of English to agree on the nature of the conditions in which it can be used. We may begin our analysis of the sentence with an analysis of its words, one at a time, beginning with MAY.

The only syntactic information we will need to appeal to is the information that the sentence is a question, that its subject is the pronoun WE, that its main verb is COME, that it contains, in association with this verb the modal auxiliary MAY, and that the verb COME has a directional complement IN. The word MAY, when used as a modal auxiliary, has three functions that will interest us here, and these are separable as its (1) epistemic, (2) pragmatic, and (3) magical functions. In its epistemic function, it is used in connection with likelihood-estimating expressions such as HE MAY NOT UNDERSTAND YOU. In its pragmatic function, it is used in sentences utterable as parts of permission-granting or permission-seeking acts, such as YOU MAY COME IN NOW. In its magical function it is used in the expression of wishes, blessings and curses, such as MAY ALL YOUR TROUBLES BE LITTLE ONES or MAY YOU SPEND ETERNITY ROLLER-SKATING ON COBBLESTONES.

The magical function appears only in sentences with initial MAY, but there only if the sentence is not construed as a question. YOU MAY SPEND ETERNITY ROLLER-SKATING ON COBBLESTONES may count as a warning or a gloomy prediction, but not as a curse. Our sentence, MAY WE COME IN?, is a question and does not allow the magical interpretation of MAY.

The epistemic and pragmatic functions of MAY can be seen ambiguously in certain sentences, as for example in JOHN MAY LEAVE THE ROOM. The person who utters that sentence may either, in doing so, be authorizing somebody named JOHN to leave the room, or he may be expressing his belief in the possibility of that peron's leaving the room at some time in the future.

The epistemic and pragmatic senses are not appropriate to every use of MAY, however; it happens that the two uses of the modal are associated with two grammatically quite distinct sets of contextual possibilities, and instances of ambiguity with respect to these two senses are instances of accidental overlap of these two context sets. I will content myself with merely giving examples: it is probably immediately clear that the permission-granting sense is completely absent from JOHN MAY HAVE LEFT THE ROOM, and that the possibility-expressing sense is absent from MAY JOHN LEAVE THE ROOM? By that I mean that JOHN MAY HAVE LEFT THE ROOM does not permit a pragmatic interpretation, as, say, I HEREBY GIVE JOHN PERMISSION TO HAVE LEFT THE ROOM, and the question MAY JOHN LEAVE THE ROOM? does not permit an epistemic interpretation, as, say, IS IT POSSIBLE THAT JOHN WILL LEAVE THE ROOM?

These observations are to be accounted for by noting that the pragmatic sense of MAY simply does not show up with the so-called perfective construction, and that the epistemic sense does not show up in questions. It was because our sentence MAY WE COME IN? is a question that we knew it had to do with permission-granting.

The recognition that the sentence is a question, then, rules out, for interpreters of MAY WE COME IN? the possibility that MAY is used in either its epistemic or its magical senses. We are left with the assumption that it is used in its pragmatic sense, and therefore that it is used in a social situation involving permission-granting in some way. Permission-granting situations involve two parties, the person or persons accepted as having authority to grant the permission, and the person or persons whose actions are to be authorized. A sentence with pragmatic MAY may be uttered performatively, in which case the utterance is a part of a permission-seeking or permission-granting act, or it may be uttered non-performatively. In the latter case, it is a statement or question about somebody's having permission to do something. It is the non-performative use of pragmatic MAY which is taught in the classroom as the preferred way of speaking of permission-possession. This use is quite unnatural to most speakers of English and will be ignored here. For persons who have been influenced by the classroom tradition, our sentence has the possible interpretation HAVE WE BEEN GIVEN PERMISSION TO COME IN?, the interpretation, in other words, which would be associated, in the speech of most of us, with the question CAN WE COME IN?. In what follows I will be disregarding the non-performative sense of pragmatic MAY.

In a performative utterance of a pragmatic MAY sentence, the possessor of authority is taken to be the speaker if the sentence is an assertion, the addressee if the sentence is a question. Thus, the speaker of JOHN MAY LEAVE THE ROOM is, in saying the sentence, authorizing John to leave the room. The sentence we are examining, however, is a question, and in uttering a question with pragmatic MAY, the speaker is acknowledging the addressee's authority with respect to the permission-granting gesture. This alternation of the authority role between the speaker of an assertion and the addressee of a question must be accounted for in terms of general principles of conversation and general principles in the logic of questions and answers.

Without touching on the details, it is at least clear, for the rules of two-party discourse, that the speaker and addressee roles alternate in ways exhibited in such exchanges as I DID A GOOD JOB / NO YOU DIDN'T or HAVE YOU SEEN HIM? / YES, I HAVE. In general, if A asks B a question, A acknowledges B's authority to answer the question, and B in trying to answer the question, acknowledges that acknowledgement. Any of the ways in which the A's sentence assigns separate roles to speaker and addressee must have those assignments reversed in B's contributions to the same conversation. In a sentence like JOHN MAY LEAVE THE ROOM, the speaker of that sentence is the authority with respect to the permission-granting act which a performance of that sentence may constitute. If that sentence is, as it is, an authorized answer to the question, MAY JOHN LEAVE THE ROOM?, it follows that the addressee of the question has the same role as the speaker of the corresponding assertion.

So far, then, we have seen how a speaker of English is able to reach certain conclusions about our sentence: from the fact that it is a question and contains the modal MAY, (1) it involves the permission-granting sense of MAY, and (2) it is the addressee of the sentence who is taken as having the right to grant the desired permission.

Recall that I have pointed out that assertions containing pragmatic MAY could be uttered as, or as a part of, or as constituting, permission-granting acts. Because of that fact, my use of the term 'assertion' was not quite appropriate--though I will not abandon it since I have not found a suitable alternative. An utterance of YOU MAY COME IN is not merely a statement declaring or asserting that some state of affairs exists--it is rather an instance of a type of utterance that has some sort of extra-linguistic validity as defined in particular sorts of social situations. It is a sentence, an utterance of which, under an appropriate set of conditions, constitutes an illocutionary act of the type that we have been referring to as permission-granting. A sentence like THE CAPITAL OF FRANCE IS PARIS cannot, in any perfectly straightforward way, be uttered as one step in a socially dynamic situation, but the sentence YOU MAY COME IN can.

We must ask, then, just what is the illocutionary force of the question MAY WE COME IN?. This question imposes on the addressee the obligation to exercise his authority. An utterance of the question, under the conditions mentioned above, is an illocutionary act whose effect is to get the other party to perform a related illocutionary act; an utterance on B's part of the answer YES or the answer NO to this question will be taken as an authorizing or as a forbidding act respectively.

I have mentioned several times the role of speaker and addressee as factors to deal with in the interpretation of utterances, and I have referred to the fact that in two-party conversations, the identity of the speaker and the addressee will systematically alternate. Aspects of the interpretation of sentences that relate to the speech act situation are known collectively as *deixis*, and reference to the participants in a speech act are covered by the term *person deixis*. When explicit reference is made in an utterance to the speaker and the addressee of the utterance, the English pronouns I and YOU are used; but we have seen from the analysis of assertions and questions with pragmatic MAY that not every appeal to the participants in a speech act involves the presence of one of these words. Other forms of *deixis*, soon to be mentioned in another context, are *place deixis*, involving reference to the location of the speech act participants (as seen in such words as HERE and THIS), and *time deixis*, involving indication, direct or indirect, of the moment of the speech act (as seen in such words as NOW and TODAY).

We turn next to the person-deictic pronoun in our sentence, the word WE. The traditional grammatical term for a linguistic form which identifies the speaker of a sentence is "first person", and the traditional label for pronouns of the type of English WE is "first person plural". This characterization is, of course, rather odd. If we identify the "first person" as the speaker--the one who pronounces the sentence containing the "first-person" form--then the description "first person *plural*" makes sense only in the case of choral recitation, speaking in unison. The English word WE has, actually, a quite different use. It identifies a group of individuals including the speaker of the sentence; it refers, in other words, to the speaker of the sentence and somebody else.

In many languages a distinction is made in their so-called "first person plural" forms depending on whether the group does or does not include the addressee of the sentence. Such languages distinguish an inclusive form, designating the speaker and the addressee (and maybe others), from an exclusive form, designating the speaker and one or more beings not including the addressee.

Linguists find it necessary to speak of the inclusive and exclusive uses of the English pronoun WE. In translating from English into a language which makes this distinction explicit, one needs, obviously, to assign one or another "clusivity" value to each instance of the pronoun. In descriptions of the speaker/addressee alternation in conversations, different values of WE must be assigned to the first contribution to each of the following conversational exchanges: DID WE MAKE A MISTAKE?/YES, WE DID. DID WE MAKE A MISTAKE?/YES, YOU DID.

In the sentence MAY WE COME IN?, it is clear that the WE is exclusive, and that was in fact the reason we were forced to imagine three beings in the situation calling for this particular utterance. The individual we have been calling C is the "somebody else" included in the scope of WE and distinct from the addressee, B. In the permission-granting situation, the person with authority and the person or persons seeking permission, are necessarily distinct, as we have seen. Since in a question with pragmatic MAY the addressee is the one with the authority, the addressee cannot be included in the scope of the subject expression.

So far, this is what we know: from the fact that our sentence is a question having MAY as its main modal, we know that is has to do with a permission-requesting situation; from the fact that it is a question rather than an assertion, we know that the addressee is assigned the authority role; from a general understanding of permission-granting situations, we know that the person having authority is distinct from the persons who need and seek permission to enter, and therefore that the pronoun WE is used in the sense which is exclusive of the addressee.

We turn now to the third word, the word COME. We notice first of all that it is an action verb, and therefore the activity it identifies qualifies as something for which it makes sense to speak of granting permission. If our sentence were something like MAY WE UNDERSTAND YOUR PROPOSAL?, we would have had to reject it as a well-formed pragmatic-MAY question, since one does not speak of needing permission to understand something. As an action verb, furthermore, it is not an "achievement" verb. If our sentence were MAY WE SUCCEED ON THIS PROJECT?, it would have to be rejected as a pragmatic-MAY question, since SUCCEED, as an achievement verb, refers to carrying out an activity which leads, fortuitously, to a particular consequence; and one does not speak of needing permission to have good luck.

The verb COME, secondly, is lexically simple with respect to the type of activity it designates. In this way it is unlike a verb like SWIM, which has associated with it both the idea of motion and an understanding of the manner of motion. If our sentence were MAY WE SWIM IN?, we would have had to point out that this sentence can be used under two distinct conditions in a permission-seeking situation. Suppose, for illustration, that the speaker and his companions were swimming in a body of water that entered a cave, and they were addressing a person guarding the entrance to the cave. In that case, there is no question of their needing permission to swim, they are asking for permission to move into the cave while swimming. The sentence, in that case, would have heavy stress on IN. Suppose, on the other hand, that the speaker and his companion have already been granted permission to enter the cave, and they wish to know whether they may do this by way of the stream, that is, by swimming, rather than by using some other means. In that case, it is understood that they have permission to move into the cave, and what they are seeking is permission to do this by swimming. In that case the sentence would have heavy stress on SWIM. The verb COME, I suggested, does not have this sort of lexical complexity, and so there is not the same sort of ambiguity with respect to which aspects of the situation are those for which permission is needed.

The verb COME, however, has other sorts of complexities. The description of the presuppositional structure of this verb requires reference to all three types of deixis--person, place, and time.

For speaking of temporal matters in the semantics of natural language sentences, it is necessary to distinguish--on the simplest level--the time of the speech event, on the one hand, and what we might call the time of focus, the time that is being referred to or focused on in the sentence. We can see how both of these types of references can figure in the description of a single sentence by considering a sentence like JOHN WAS HERE LAST TUESDAY. The time of focus is identified by the phrase LAST TUESDAY, and the time of the speech act is involved in the interpretation of the word HERE: HERE is the place where the speaker finds himself at the time of his pronouncing the sentence. (Even for fairly simple cases, it is necessary to distinguish more than just these two temporal reference points, but for the points I have in mind, these two will do.)

The role of deictic categories in the interpretation of sentences with COME may be observed with sentences of the form "X" CAME TO "Y" AT "T", where "X" is the moving entity, "Y" is the destination, and "T" is the time of focus. (Here, "T" is taken to be in the past, just for the sake of simplicity.) It happens that sentences of the form "X" CAME TO "Y" AT "T" are appropriate just in case any of the following conditions obtains:

1) the speaker is at Y at the time of the speech act
2) the addressee is at Y at the time of the speech act
3) the speaker was at Y at T
4) the addressee was at Y at T

To see that this is so, take JOHN, THE OFFICE, and YESTERDAY MORNING as values of "X", "Y" and "T" respectively. A sentence like JOHN CAME TO THE OFFICE YESTERDAY MORNING is appropriate under any of the four conditions just indicated. It is a sentence I can say appropriately if I am in the office when I say it, if you are in the office when I say it to you, if I was in the office yesterday morning when John came, or if you were in the office yesterday morning when John came. (There are uses of the verb COME in pure third-person narrative which are not covered by the description, and there are personal identifications with places distinct from physically being in a place for which COME is nevertheless appropriate; but on these matters the reader's indulgence is requested.)

Limitations on these appropriateness conditions appear when we substitute for the "X" and "Y" of the formula expressions of person deixis and place deixis respectively. For example, if I say I CAME THERE YESTERDAY MORNING, it is not possible that I am "there" now, because THERE is by definition a place where I am not now located; and it is also not possible that I was already there yesterday morning when I came.

But now what are we to say about our sentence, MAY WE COME IN? We have seen, from the fact that we are dealing with permission-granting MAY in an interrogative sentence, that our pronoun WE is exclusive of the addressee. That same conclusion could also have been reached by noticing its occurrence with the verb COME. COME is a verb of locomotion which indicates a change in location from some point of origin to some destination, this latter conceived of as a place where the speaker or the hearer is at the time of the speech act or was at the time of focus. In a permission-seeking utterance with the modal MAY, there is lacking a definite time of focus, and that leaves open only those possibilities that refer to the participants' location at the time of the speech act. Since the pronoun WE has to include the speaker and does not have to include the addressee, we are forced to include that WE is exclusive; since the addressee must be at the place of destination in order for the use of this sentence to be appropriate, he cannot be included in the group seeking to move toward that destination. Again our analysis supports the picture we had at the beginning, of A, on the outside, speaking on behalf of himself and C, also on the outside, and addressing the insider B.

The verb COME, like its partner GO, is one of the few verbs of motion in English that require a destination complement in syntactically complete sentences. In our case the destination complement has the form IN, which we may take as an ellipsis for IN(TO) THE PLACE, or the like. The particle IN ascribes to the destination the information that it is some sort of enclosure. That information, together with the change-of-location interpretation required of the verb COME, is what imposes the understanding that the moving entities have as their point of origin a location which is not within that enclosure. Notice that quite different assumptions about the relative position of speaker and addressee would have been taken if the sentence had been MAY WE COME UP? or MAY WE COME THROUGH? or the like.

So far we have examined certain properties of the individual words of the sentence. In doing that we have fairly exhausted, because of the simplicity of the sentence, whatever there is to say about its grammatical structure as well, at least its "surface structure". There are current arguments in support of the claim that the deep structures of sentences will correspond to their fully specified semantic descriptions--including an account of their illocutionary force--but since my effort here is to uncover and detail such a semantic description, it will not be necessary to adjoin to my discussions any specific claims about the nature of the deep structure.

The illocutionary act potential of the sentence must be studied in the context of the systems of rules or conventions that we might choose to call discourse rules, a subset of which might be called conversation rules. We have seen, in what has already been said about the illocutionary force of our sentence, that it is not to be construed as a request for information, but as a request for the addressee to "perform" in some way. It is usable as a way of getting the conversational partner to perform the needed permission-granting (or, of course, permission-denying) act. In the sense that a question like SHALL WE COME IN? is an utterance spoken to get one's interlocutor to issue an order, the question MAY WE COME IN? would be spoken to get the addressee to grant permission. Because of its role in a changing interpersonal situation, a complete description of the sentence must specify the various social conditions which must be satisfied in order for it to be used appropriately. For various reasons, these may be stated as belief conditions which must be satisfied by the utterer of a sentence in order for us to acknowledge that he has uttered the sentence in good faith. We have agreed, for example, that the speaker must believe that his interlocutor is inside E, that he and his companion are outside E, and that his interlocutor is a person capable of authorizing admission into E. By viewing these as belief conditions, we are able to recognize various ways in which the sentence may be uttered deviantly. It may be uttered mistakenly, in case the speaker's beliefs are incorrect; or it may be uttered insincerely, in case the belief conditions are not satisfied.

The speaker may be mistaken in his belief that he is outside the enclosure E; he will realize his mistake if his question is answered, WELL, IT LOOKS TO ME LIKE YOU'RE ALREADY INSIDE. He may be mistaken in his belief about the location of the addressee; he will realize that if he hears, from an unexpected direction, the reply, YOOHOO, HERE I AM. GO RIGHT ON IN. He may be mistaken in his beliefs about the relative social positions, with respect to this activity, of himself and his addressee. It may not be necessary for him to receive permission to enter, as he will find out if the answer is, OF COURSE. The person he is addressing may not have the authority to admit him, as he will learn if he hears the answer, DON'T ASK ME.

The sentence can be used "insincerely" in either of two ways. It may be used politely, in which case the assumptions associated with the sentence about the social dominance (on this occasion, at least) of the addressee, are intended as a polite social gesture; or it may be used ironically, as in cases where the suggested dominance relation is clearly contradicted by the realities of the situation. An example of an ironic use can be seen in a situation in which a prison warden addresses the question to a prisoner in his cell, or in the case of a pair of aggressive encyclopedia salesmen who have already entered the living room.

The conversation rules of a language govern not only the conditions under which it is appropriate to perform a permission-requesting utterance of the type we have been examining, but they must also determine the principles by which a speaker of English is able to recognize appropriate responses to the request. If the question is used in absolutely its most straightforward way--a rare occurrence, I would guess--the normal appropriate answers would be merely YES, YOU MAY or NO, YOU MAY NOT. Such an answer merely acknowledges that B does indeed have the requisite authority and that he does or does not tolerate the entering into his territory of A and C.

If, however, the question was asked in the way it was asked out ot politeness, as an instance of the sort of social gesture that occurs in conversations between equals or near-equals, and if B recognizes and wishes to return the gesture, he would say something like YES, PLEASE DO or COME IN, BY ALL MEANS. These answers, having the form of imperatives, have associated with them some of the conditions appropriate to imperative sentences--one of these being that it is in the speaker's interest to have the addressee act as commanded. Thus, on being asked whether one would tolerate admission into a room, the person who responds with YES, PLEASE DO shows that he not only tolerates but desires such a move

From observations such as these, it is now obvious that the rules of conversation must not only specify the appropriateness conditions for an utterance and the nature of the most straightforward appropriate second-speaker responses to utterances, but must be capable of making use of certain logical operations by which it can be shown that something equivalent to a straightforward response is deducible from the actual occurring response. In particular, such principles would include the information that desiring implies tolerating, that necessary implies possible, etc.

Let me now summarize the various kinds of facts which must, I suggest, be included in a fully developed system of linguistic description.

The linguistic description of a language

(1) must characterize, for each lexical item in the language
    (a) the grammatical constructions in which it can occur,
    (b) the grammatical processes to which it is subject in each relevant context,
    (c) the grammatical processes which its presence in a construction determines, and
    (d) information about speech act conditions, conversation rules, and semantic interpretation which must be associated in an idiosyncratic way with the lexical item in question;

(2) it must provide the apparatus which characterizes
    (a) the grammatical structures of sentences on the "deep" or abstract level, and
    (b) the grammatical processes by which abstract linguistic structures are processed and become surface sentences;

(3) it must contain a component for calculating the complete semantic and pragmatic description of a sentence given its grammatical structure and information associated with each lexical item;

(4) it must be able to draw on a theory of illocutionary acts, in terms of which the calculations of (3) are empowered to provide a full account of the potential illocutionary force of each sentence;

(5) it must be able to draw on a theory of discourse which relates the use of sentences in social and conversational situations; and

(6) it must be able to draw on a theory of "natural logic" by means of which such judgments as the success of an argument or the appropriateness of elements in conversations can be deduced.

OPERATIONAL PROBLEMS OF MACHINE TRANSLATION:

A POSITION PAPER

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The late Uriel Weinreich in the first of his lectures in the Trends in Linguistics lecture series at the Indiana University Linguistic Institute on July 13, 1964 referred to machine translation as "linguistics' most conspicuous and expensive failure."[1] Two years later the Automatic Language Processing Advisory Committee of the National Academy of Sciences, National Research Council in what has since become known as the ALPAC Report (1966: 24) stated that "No one can guarantee, of course, that we will not suddenly or at least quickly attain machine translation, but we feel that this is very unlikely."

In the light of these two highly authoritative statements of position, and in view of the abrupt reduction of funding for machine translation research, is it at all reasonable to discuss operational problems of machine translation these days? The answer is of course that if one is to talk about machine translation at all, it must be in terms of some reasonable operational objective, since research without such objectives will at best be related to machine translation only indirectly. The question as to whether or not such objectives are reasonable depends in this author's opinion upon the researcher's basic orientation: with a predominantly theoretical orientation, machine translation research will clearly be close to pointless; with an operational orientation, on the other hand, machine translation research will not only be interesting and valuable in its own right, but will also constitute one of the few available conclusive means of verification of the findings of linguistics (cf. Garvin 1962: 387).

This paper will attempt a survey of the major controversial issues in the field of machine translation, all of which, in the light of the above discussion, are considered operational. These issues are considered to fall into three basic categories: linguistic problems, design problems, and bread-and-butter problems.

LINGUISTIC PROBLEMS

The Role of Linguistics Theory in Machine Translation

It is a commonly held view among linguists, both the few who are interested in machine translation and the many who are not, that any application of linguistics--and in the linguist's view this certainly includes machine translation research--must be based primarily on a strong linguistic theory. (For a recent statement of this view see Bar-Hillel 1970). This is essentially a capsule view of the theoretical orientation. While nobody will deny that any applied work must have a sound theoretical basis, from an operational standpoint there are a number of things seriously wrong with an over-emphasis on theory.

(1) Machine translation is considered primarily an operational rather than a theoretical problem. Consequently, an application of sound linguistic research methods is more important than a further elaboration of linguistic theory.

(2) Most strong linguistic theories are essentially generative in nature. However, the basic problem in machine translation is not a generative but a recognition problem. Recent research in psycholinguistics has confirmed an opinion long held by this investigator, namely that a recognition problem cannot be resolved by simply reversing a generative system.

(3) The kind of questions that arise in machine translation research are not necessarily the kind of questions that are most popular among linguistic theorists. More specifically, the basic problems of ambiguity resolution are different in machine translation from the formulation they have received in

recent explanatory linguistic theories.

(4) It is an old operationalist adage that one can best learn by doing. This is particularly true in the case of machine translation where the machine manipulation of linguistic data forces the investigator to recognize a great many inaccuracies and intuitive shortcuts that are usually glossed over in theoretical linguistic research. Thus, rather than relying excessively on the contributions of linguistic theory to machine translation research one should expect significant contributions to linguistics from research on machine translation.

Nature of Models

Linguistic models can be categorized as strong or weak, depending on whether or not they have strong or weak formal pretensions. Current trends in linguistics favor strong models; this is of course based on an epistemological attitude that is oriented towards the elaboration of theory rather than of method. In line with the discussion in the preceding section, it is here considered that, particularly for purposes of an application such as machine translation, weak models are to be preferred to strong ones. The reason is that strong models are considered to prejudge the direction of research in a situation in which there are too many unforeseen and as yet insufficiently known factors. Clearly, there have to be grammars of both the source and target languages at the base of any machine translation system. Equally clearly, however, these grammars need not be formal grammars; as a matter of fact, in this author's opinion descriptive grammars are strongly preferable to formal grammars for purposes of machine translation because they are much better able to account for the indeterminacies of natural language structures which, as was so well stated by Charles F. Hockett recently, are essentially ill-defined systems (Hockett 1968: 44-45). Descriptive grammars can best be developed in a primarily method-oriented, rather than a primarily theory-oriented, frame of reference. As a matter of fact, in such a frame of reference conventional grammars may be used as a reasonable point of departure, with the necessary modifications introduced as the requirements of machine translation become apparent in the process of the development of experimental systems. Operationally oriented machine translation research both in the United States (cf. Garvin forthcoming) and in the Soviet Union (cf. Bel'skaja 1969) has done just that. This author has made strong claims on behalf of his proposed version of an operational machine translation system (Garvin 1967); it is not known how far along comparable Soviet versions have progressed.

Aspects of Linguistic Structure

All linguists seem to agree that the system of language is hierarchically structured. That is, they all look upon the system of a language as having different levels, or strata, or components. From a machine translation standpoint, of course, it is most important to know which distinctions between different aspects of language are relevant for the development of machine translation systems. The least significant seems to be that of phonology and grammar, since no machine translation system to this author's knowledge is concerned with phonology at all. The most important is the distinction between grammar and lexicon, since all machine translation systems known to this author make some distinction somewhere between a dictionary lookup based on the lexicon and an algorithmic portion based in part on the grammar.

Linguistic approaches differ in regard to whether or not the lexicon is considered a part of the grammar or a dimension separate from it. In either case, the lexicon and the grammar (or the remainder of the grammar) are kept clearly separate by most linguists. The difficulty in machine translation is that the lexicon and the grammar cannot be hermetically sealed off from each other. The dictionary and the algorithmic portion correspond only roughly to lexicon and grammar respectively; the dictionary, after all, contains a grammar code which is based on the grammar, and the algorithmic portion serves to resolve not only grammatical but also lexical ambiguities. Nevertheless, an understanding of the differences between lexicon and grammar is essential for a proper operational assessment of all the variables that enter into the design of a machine translation system.

Related to the conception of levels or strata of language is the methodological problem of conducting the analysis "from the botton up" or "from the top down". In the first case, the minimal units of language are considered as the input into the analysis and the output yields the maximum units which are, for all practical purposes, the sentences of the text. In the second case, the input are the sentences and the output is a decomposition of the sentences into their constituents. Clearly, since in machine translation the grammatical information is transmitted to the algorithmic portion from the dictionary by a lookup of individual textwords, and since therefore the initial input elements into the algorithmic portion are the "bottom units", a "bottom to top" approach is the most operationally efficient one for machine translation.

DESIGN PROBLEMS[2]

Sensing Units and Translation Units

This is one of the oldest and also most important problems faced in the design of machine translation systems. Sensing units are linguistic units which the computing equipment can read, that is, for all practical purposes, strings of letters separated by spaces and/or punctuation marks. Clearly, these correspond only partially to the translation units, that is to say, the grammatical and lexical units that must be manipulated in order to effect translation. The problem consists in providing the machine translation system with a capability for transforming the sensing units into appropriate translation units. In a sense, the entire recognition problem in machine translation is a consequence of this difference between sensing units and translation units. Were it not for that, the brute force conception that machine translation can be effected by a large enough dictionary with some adaptations to make room for syntactic and semantic differences between the two languages, would indeed be adequate. And, needless to say, everyone who has had any experience with the field knows that this is not so.

Intermediate Language

There has been a good deal of discussion in the machine translation literature about the presumed advantages of an intermediate language (for an example of this, cf. Andreyev 1967). It has, for instance, been asserted that with the help of an intermediate language one could ultimately accomplish translation from any one of a number of different languages into any one of a number of other languages much more economically than by designing a corresponding series of translation systems for the different conceivable language pairs. Some have gone so far as to give a mathematical expression to this theoretical economy. From an operational standpoint, of course, the real problem is whether or not machine translation of any satisfactory quality can be achieved at all, rather than how it can be achieved most economically for an indefinite number of language pairs.

Nevertheless, the question of intermediate language deserves serious consideration, from an operational as well as from a theoretical standpoint. Some authors, such as Andreyev (ibid.), have talked about an intermediate language as if it were a real language such as English or Russian. That is, translation would be done from the source language into an intermediate language conceived of as a real language, and then back out of the intermediate language into the target language. If the intermediate language is so conceived, then this means encumbering the translation process by an additional step: instead of translating in a one-step process directly from the source language into the target language, it becomes a two-step process in which translation is first effected from the source language into the intermediate language, then from the intermediate language into the target language.

If on the other hand the intermediate language is not conceived of as a genuine language with all the appurtenances, then the conception of an intermediate language becomes much more rational from an operational standpoint, and also much more trivial from a theoretical point of view. In that case, the intermediate language is nothing more than a series of symbolic notations to record the output of the recognition routine and to serve as input into the command routine by which the text in the target language is to be generated. This, as was said, is operationally effective--it is also operationally necessary, because there must be some way in which the information gathered by the recognition routine is stored and transmitted out into the command routine. The use of the term intermediate language then becomes trivial, because this information store will certainly not have the language-like qualities which the term implies. It is further conceivable from an operational point of view, although certainly premature at the present state of machine translation research, that the same information store can be filled by a number of different recognition routines for different languages, and in turn feed into a number of different command routines for different target languages. The information store then will be combined with a kind of switchboard that will direct the appropriate recognition routine into the store and make sure that the output of the store is fed into the appropriate command routine. Thus, the theoretical effeciency talked about in the preceding paragraph is conceivable, but in a sense which for the current state of affairs is operationally trivial.

Total Accountability

Many linguistically oriented researchers in machine translation have claimed that in order for machine translation to be possible, it is necessary to account for all of the linguistic conditions that exist in a language. Some, such as Bar-Hillel (1970) have gone even further and claimed that not only linguistic conditions, but also pragmatic conditions have to be accounted for in order to make machine translation of the desired quality possible.

From an operational standpoint, this is an inappropriate identification of the aims of exhaustiveness in linguistic research with the aims of machine translation. Clearly, only those linguistic conditions which have a bearing on the translation process need be accommodated in a machine translation scheme. Thus, most of derivational morphology, although of great interest to the linguistic researcher, is essentially irrelevant to the translation process, since derived forms can be entered into the machine translation dictionary with their appropriate translations without going through the trouble of underlying analysis. Similarly, it is certainly not be be expected of a machine translation system any more than of a human translator to translate unambiguously passages which are inherently ambiguous in the source language. Likewise, no machine translation system should be expected to account in its entirety for those pragmatic factors which under ordinary circumstances would remain obscure to the human peruser of the source language text.

Morphological Analysis

Quite a few linguistically oriented machine translation researchers have given a great deal of attention to automatic morphological analysis as part of the machine translation process. This analysis has been primarily concerned with attempting to determine morpheme boundaries within printed words; some researchers have limited themselves to separating inflectional endings from the base portions of the words, while other researchers have gone further than that and also included the segmentation of derivational morphological material. One of the reasons given for this has been the requirement of total accountability which was discussed in the preceding section. Another, operationally more valid, reason has been that separating inflexional endings from base portions, while it may encumber dictionary lookup, saves a great deal of storage space in the dictionary portion of the program. The reason given for segmenting derivational material has been that it facilitates the recognition of neologisms. Clearly, the latter two reasons apply primarily to "highly inflected" languages such as Russian or German.

As far as the segmentation of inflectional morphemes is concerned, which some machine translation groups have called "stem-affixing", this is a perfectly reasonable space-saving procedure when it comes to high frequency regular inflectional patterns. In the case of the so-called exceptions, particularly when the irregularities involve changes in the base portions of the words, no operational gain is derived from the segmentation of inflectional morphemes from base portions.

As far as the segmentation of derivational elements is concerned, the advantages derived from the facilitation of the recognition of neologisms have to be weighed against the disadvantages of introducing an additional elaborate systems task into the design. In this author's opinion, the segmentation of compounds into their components may well be extremely useful in the recognition of neologisms. On the other hand, the segmentation of derivational morphemes from the remainder of the base portions of the words is both operationally more cumbersome than the segmentation of compounds, and less likely to yield results in the correct recognition of neologisms. It is, after all, well known that the lexical meanings of derived words, particularly in the Slavic languages, are often not predictable from the sum of the meanings of the derivational morpheme or morphemes and the remainder of the base portion.

Grammar Code and Algorithmic Portion

Most workers in the field of machine translation agree that grammatical information is stored in the form of grammar codes in the dictionary of the system; the term grammatical information is here used loosely to include whatever lexical and other semantic information is available to the program. This information is then called by the algorithmic portion of the system for further processing to effect the required recognition of the source language input and subsequent generation of the target language output. This raises the question as to how much information is to be stored in the grammar code, and how much of the recognition and subsequent generation task therefore is to be left to the algorithmic portion. The current trend in much of linguistic theorizing has been to emphasize the significance of rules; this means, from a machine translation standpoint, that a great deal of the recognition burden is placed on the algorithmic portion, with only as much contained in the grammar code as is considered theoretically desirable. Since, however, a table-lookup operation is significantly easier to perform than an algorithmic one, there is a distinct operational advantage in maximizing the grammar-coded information and minimizing the role of the algorithmic portion. This does not of course mean that under any circumstances the role of the algorithmic portion becomes as trivial as some early machine translation researchers have assumed it to be. The other question, namely, whether or not the algorithmic portion should contain a separate table of rules, will be discussed in a subsequent section on bipartite and tripartite design in machine translation.

Content of the Grammar Code

One of the lessons learned by all machine translation research groups has been that the amount and type of grammatical information contained in conventional grammars is wholly inadequate for machine translation purposes. At least the following types of information have been found essential for most full words in a language such as Russian: (1) Word class information. This includes not only indication of the conventional word class, but requires a reformulation of word class distinctions in terms of syntactic functioning of Russian words. Thus for instance, instead of including participles under verbs as one of their forms, as is done in many traditional grammars, participles are considered a special type of attributives with the particular function of having the potential for governing dependent structures. Thus participles are included in a category of governing attributives together with a number of adjectives that are functionally equivalent to them, and are not included in the same class with verbs. Similarly, infinitives are considered a separate word class because, unlike finite verbs, they do not ordinarily have subjects, and unlike gerunds which likewise do not have subjects, they may serve themselves as subjects of Russian clauses. (2) Agreement information. This has to include information with regard to modifier—head-type agreement, as well as information as to whether or not the agreement is of the ordinary type (as for Russian adjectives) or of the exceptional type (as for Russian numerals). (3) Modifiability information. This information has to state what dependent words a given word may be modified by. That is, in the case of nouns, what adjectives may modify a given noun; in the case of adjectives, what adverbs may modify a given adjective; etc. (4) Complementation information. This information concerns the type of complements which may be associated with a given verb, noun, or other Russian word that may have a complement in association with it. Thus, verbs or nouns of location may have certain types of complements of place accompanying them; verbs or nouns of time may have certain complements of time accompanying them, etc. (5) Governor class information. For those words which may be governed by other words, information in regard to the particular kinds of words which may govern them: for instance, in the case of adjectives, the kind of nouns to which they may be modifiers; in the case of adverbs, the kind of adjectives to which they may be modifiers. (6) Government information. Those words which govern dependent structures, the kind of dependent structures which they may govern. For instance, the kind of case a verb or noun may govern, whether or not more than one dependent structure may be governed and in which case each of the possible dependents will stand, whether or not there is prepositional government (which preposition and demanding which case), etc. (7) Subject class informatio For verbs, the class of subjects which a given verb may take, such as animate, inanimate, human, etc. (8) Object class information. The same type of information as for subject class, except of course, concerning the object which a given verb or a given governing attributive may take.

The above includes only a part of the kind of information required for a complete grammar code. Much of this information is commonly considered semantic rather than grammatical; much of it has to do with not only the syntactic recognition of the sentence but also with the recognition of semantic compatibilities. A great deal more information is needed if in addition to this type of recognition correct choices are to be made in the case of multiple meaning.

Bipartite versus Tripartite Design[3]

The issue here is whether or not the rules of the grammar of the source language should be contained in a table to be called by a parsing algorithm, or whether these rules should be written into a more elaborate algorithm of which they become an organic portion. In the first case, the machine translation program would essentially consist of three portions: a dictionary, a parsing algorithm, and a table of rules--hence, the term tripartite. In the second case, the machine translation program will consist of only two portions: a dictionary, and a translating algorithm--hence, the term bipartite.

The main arguments in favor of a tripartite design are: (1) that it allows the processing by one and the same parsing algorithm of more than one table of rules; thus, if any corrections in the grammar are to be made, this involves only a relatively simple updating of a given rule table, and does not require any revision of the algorithm itself; (2) the labor of the programmer who is responsible for the parsing algorithm can be kept separate from the labor of the linguist who is responsible for the table of rules. In theory, these two advantages appear to be overwhelming. In practice, it turns out that the fundamental problem in the automatic recognition of grammatical structure of text is the correct sequencing of the application of the rules of the grammar which are supposed to effect the recognition. In this author's opinion, such a sequencing of the application of different grammatical rules can be effected only by making the rules of the grammar an organic part of the algorithm; this is the only way to insure that a given rule will be called only after all the conditions that are necessary for its operation have been previously recognized by other rules of the program, and that such a recognition has been effected in the correct order.

This requirement of sequencing of rule application is based not only on the recognition that the grammar of a language is hierarchically structured, that is, that there are levels to be gone through. It is also based on the recognition that in addition to the levels of the language, there is also an operational order in which grammatical and other information becomes available to the program. Thus, once again, it is apparent that the operational requirement does not parallel the theoretical desiderata.

Recognition Strategy

As was stated above, a bipartite machine translation design is considere operationally preferable to a tripartite one. This means that the algorithmic portion of a machine translation program operates on the basis of something like a pattern recognition strategy, rather than a parsing strategy. This means that the algorithmic portion will in essence carry out a number of context searches to recover the conditions necessary to effect recognition and subsequent translation.

This conception of recognition strategy raises the basic problem of the proper organization of the searches in a bipartite system. Two basic types of search patterns have been proposed in the literature: one is predictive analysis, devised by Ida Rhodes of the National Bureau of Standards and soon thereafter adopted by the then Machine Translation Research Group at Harvard University (Oettinger and Sherry 1961); the second is the author's fulcrum approach (Garvin 1968). Predictive analysis employs essentially a straightforward left to right search pattern. The algorithm looks at the grammar code of each textword, as it has been looked up in the dictionary, from left to right. For each textword it records its grammatical potential in the form of predictions and notes the extent to which predictions of previously noted words are fulfilled by each current word. If this succession of predictions and their fulfillments does not result in an appropriate recognition of the syntactic structure of the sentence, then unfulfilled predictions and unused fulfillment possibilities are retested in a program component called hindsight. The basic difficulty of this approach is that the more complex a sentence, the greater the burden placed upon the hindsight; from an operational standpoint, the greatest weakness of this approach has been that the hindsight has never properly been worked out. In the fulcrum approach, on the other hand, searches are designed to use words in order of their grammatical significance, rather than in the linear order of their appearance in text. Thus the searches are directed first at those words which contain the most grammatical information from the standpoint of the recognition of a particular structure (the so called fulcra), then they branch out from these pivot words in order to encompass the remainder of the structures in question. Since not all grammatical information is retrievable in a single pass, the fulcrum approach uses a succession of passes for the retrieval of the grammatical information contained in each sentence.

The reasons for which the fulcrum approach is considered operationally preferable to predictive analysis are the same for which a bipartite system is considered operationally preferable to a tripartite one: the need for the appropriate sequencing of the application of grammatical rules to the elements of the text.

Single versus Multiple Interpretation of Sentences

Many approaches to automatic syntactic analysis, whether connected with machine translation or not, favor the outputting of as many parsings of each sentence as is conceivable in terms of the given grammar code. The reason for this preference is the theoretical interest of showing the variety of conceivable analyses based on a given grammar code. From an operational standpoint, this is clearly undesirable, since the operational aim of machine translation is not to show the variety of conceivable interpretations of each given sentence, but to arrive at some reasonable form of translation with the minimum of waste motion. Thus, in an operational approach to machine translation priority must be given in each case to the most likely interpretation of any given sentence in the hope that this will indeed turn out to be the interpretation applicable in the particular case. As the machine translation system is refined, provisions can be included for superseding this most likely interpretation in favor of a less likely one, if the latter turns out to be the one applicable to the particular case.

Filter versus Heuristics

This question is closely related to the one treated in the preceding section.

A program component called filter has been used in some of the Soviet approaches to operational machine translation (cf. Mel'čuk 1964, Iordanskaja 1967). The intention of these systems is essentially to produce if not all, then a number of, the different possible syntactic interpretations of each sentence, by means of an algorithm which incorporates numerous table lookups and is essentially based on a variant of dependency grammar (cf. Hays 1964). It is not known to what extent this approach has been operationally successful; it is known, however, that the Mel'čuk group has since turned its attention to other problems of a more theoretical nature (cf. Mel'čuk and Žolkovskij 1970).

A machine translation design which gives a preferred single interpretation to each sentence obviously does not need a filter for the selection of one alternative from among many. What it does need is a capability for the revision of the one selected single alternative, in case overriding conditions in the grammatical makeup of the sentence require that it be superseded by another interpretation. The mechanism for overriding previously made determinations as to the interpretation of sentences is given by the inclusion of a heuristic capability in the machine translation design. The initial preferred interpretation of a sentence is given on the basis of information derived early in the syntactic processing. This information may have to be overridden on the basis of more powerful information obtained at later stages in processing. Consequently, the heuristic component must both recognize which interpretations may be subject to later revisions, as well as identify the conditions on the basis of which any prior interpretation is subject to such a revision. Usually, the original interpretation is arrived at on the basis of the immediate context, and whatever revisions may be necessary arise from the inclusion of a broader, usually clause-wide, context. The advantage of combining a single preferred interpretation with a capability for revision based on heuristics is essentially that in most cases the original preferred decision, precisely because it is based on greater likelihood, may be allowed to stand. Thus a great deal of the processing involved in the use of filters can be avoided. (For a detailed discussion of the use of heuristics in the fulcrum approach, see Garvin 1968: 172-81).

BREAD-AND-BUTTER PROBLEMS

Quality of Translation

A great deal of discussion in the machine translation literature has been devoted to the feasibility or non-feasibility of high-quality machine translation. Much of this discussion has been quite unrelated to reality, because it has been based on an A Priori abstract conception of what constitutes high quality translation. Clearly, the question of the quality of translation has to be related to user need: the greater the need, the more it is possible to compromise with quality. This has recently been recognized even by Bar-Hillel (1970). For many purposes, machine translation output will be only casually scanned rather than carefully read; from a great mass of documents so perused a few may then be selected for later, more careful, human translation. Another factor to be considered is the speed with which machine translation can be effected, as compared to the time required to produce good quality translation by human labor. This has, of course, been used as an excuse for the perpetuation of operating, though operationally unviable, machine translation systems. Nevertheless, it is one of the practical problems deserving more careful consideration than has been afforded them in the past.

Input Preparation

In the view of most observers, the greatest practical handicap in the use of machine translation has been the high cost of key-punching the original document for input into the computing system. Clearly, the only way of overcoming this handicap is by the use of automatic character recognition.

Recent claims to the effect that character recognition is now feasible for a sufficient number of fonts to be practical seem to have some validity. Undoubtedly, this will have a great effect on the evaluation of the economics of machine translation in the future, provided the question can be approached with sufficient detachment from the mistakes of the past.

3

Staffing

This is the most complex practical problem in both machine translation research and in the maintenance and updating of the machine translation systems of the future, if indeed such systems will ever become practical.

The reason this is such an extremely difficult problem is because both the development and the maintenance of machine translation systems require the cooperation of personnel with two sets of qualities that are very rarely found in the same individuals. On the one hand, work in machine translation requires great originality, expertise, intuitive brilliance, and all the other qualities that make for good researchers. On the other hand, machine translation research also requires extreme intellectual discipline, patience, persistence, and willingness to give up one's individual original ideas in favor of the established parameters of the system.

One of the more easily resolvable problems of staffing is the decision as to whether the work of linguists and programmers should be combined in the same persons, or whether the two competencies should be kept separate. In this author's experience, no linguist will ever become a good enough programmer, and conversely. Therefore, in order to maintain the highest possible level of professional competence in the research staff, the two competencies should be kept separate but should learn to work in close coordination. This again is an extremely difficult objective to achieve in practice, although it is much talked about in theory.

Footnotes

[1] This statement was not included in the published version of his lecture (Weinreich 1966).

[2] For a discussion of the design of the author's proposed machine-translation system, see Garvin 1968.

[3] For a detailed discussion of the author's views of this issue, see Garvin 1966.

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The Logic of English Predicate Complement Constructions

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The Logic of English Predicate Complement Constructions[1]

Lauri Karttunen

0. INTRODUCTION. The title of my paper is an intentional variation on the name of Peter Rosenbaum's (1965) well-known MIT dissertation 'The Grammar of English Predicate Complement Constructions'. It is intended to be suggestive of a difference in emphasis between the early work on complement constructions by Rosenbaum and others, and the more recent studies by Paul & Carol Kiparsky, George Lakoff, Jerry Morgan, and myself - just to mention a few.[2] It is these newer developments that I will discuss in my report.

In the appendix to this thesis, Rosenbaum provided a classification of English verbs in terms of the complement structures in which the verbs may participate. His analysis of complementation has since been challenged, and the basic criteria for his classification have now generally been rejected.[3] But of course, the general principle of classifying verbs in terms of their syntactic properties continues to be valid. For example, it must be stated somewhere in the lexicon that verbs like order and force take sentential complements only in the presence of a real noun phrase object, but believe and realize can have complements as their objects. Or, if you prefer another terminology, realize is a two-place and force a three-place predicate. On the basis of such simple criteria, one might arrive at the conclusion that the verbs listed in (1) divide naturally into the four groups which are indicated there.

(1) (a) order(x, y, S) force(x, y, S)
(b) realize(x, S) believe(x, S)
(c) manage(x, S) decide(x, S) able(x, S)
(d) seem(S) happen(S) certain(S) odd(S) possible(S)

For instance, on syntactic grounds there are good reasons for regarding the verbs happen and seem as similar, since they both take sentential subjects and undergo many of the same syntactic transformations.

In selecting these examples in (1), I have not been quite as arbitrary as it first appears. It does not take long to notice that just those verbs which here fall into the same class on the basis of some superficial syntactic criteria turn out to be different when the same verbs are grouped on the basis of their semantic properties. At this point, you might take a look at the classification in (2), which gives a preview of what is to come, and compare it with (1).

(2) FACTIVES: realize, odd
IMPLICATIVES: manage, happen
IF-VERBS: force, certain
ONLY-IF VERBS: able, possible

Sometimes it is possible to show that there is a definite connection between the semantic properties of a verb and certain syntactic characteristics. For instance, it has been observed (Kiparsky 1968) that all of the factive verbs of the type (1d) are exceptions to the transformation that relates (3a) and (3b). Therefore, (3d) is ungrammatical.

(3) (a) It was certain that Bill was alone.
(b) Bill was certain to be alone.
(c) It was odd that Bill was alone.
(d) *Bill was odd to be alone.

However, I do not believe that the validity of the proposed classification crucially depends on us being able to find syntactic parallels for every distinction; and here I will not try to present any. For the purpose at hand, it is sufficient to demonstrate their semantic reality, to show that they actually play a part in our everyday reasoning.

1. FACTIVE VERBS. The term 'factive verb' is due to a pioneering study by Paul and Carol Kiparsky (1968).[4] An illustrative sample of these verbs is provided in (4).

(4) FACTIVE VERBS:

significant
tragic
relevant
odd

regret
ignore

resent
know
realize
bear in mind
take into account
make clear
find out

What is common to them is that any simple assertion with a factive predicate, such as (5a), commits the speaker to the belief that the complement sentence, just by itself , is also true.

(5) (a) It is odd that Bill is alone.
(b) Bill is alone.
(c) It is possible that Bill is alone.

It would be insincere for anyone to assert (5a) if he did not believe that (5b) is true. Intuitively, in uttering (5a) the speaker must take it for granted that Bill is alone; he is making a comment about that fact. The same relation holds between (6a) and (6b).

(6) (a) Mary realized that it was raining.
(b) It was raining.
(c) Mary believed that it was raining.

Notice that these relations break down if we replace *odd* by *possible* and *realized* by *relieved*. (5c) and (6c) do not carry a commitment to the truth of the complement sentence.

With factive verbs, it does not make a difference whether the main sentence is affirmative or negative. The negations of (5a) and (6a), which you find in (7), also obligate the speaker to accept the complement as true.

(7) (a) It isn't odd that Bill is alone.
(b) Mary didn't realize that it was raining.

Even the illocutionary force of the main sentence is irrelevant. The question in (8) carries along the same commitment as (5a) and (7a).

(8) Is it odd that Bill is alone?

These facts about negation and questions become important later on when we have to distinguish between factive and implicative verbs.

1.1. PRESUPPOSITION. What the above examples show is that a sentence with a factive predicate indicates belief on the part of the speaker in the truth of the complement sentence. This relation is usually described by saying that the complement of a factive predicate is a 'presupposition' for the sentence as a whole. The term 'presupposition' comes from logic but it is currently used in linguistics in a more general way than the common logical definition would actually allow. In logic, it is customary to give some definition such as (9).[5]

(9) P presupposes Q iff

$$T(P) \supset T(Q) \quad \text{and} \quad F(P) \supset T(Q)$$

[T(_) = '_ is true' , F(_) = '_is false']

That is, P presupposes Q just in case Q is true whenever P has a truth value.

However, this definition in terms of truth values is not very helpful to linguists. They tend to rely on a more or less intuitive notion of presupposition, which I have tried to explicate in (10) - rather unsuccessfully, I must say.[6]

(10) P presupposes Q just in case that
if P is asserted, denied, or questioned
then the speaker ought to believe that Q.

1.2. POSSIBLE WORLDS. In his paper on presuppositions, Jerry Morgan (1969) pointed out that there are sentences such as the examples in (11).

(11) (a) If I had missed the train, I would have regretted it.
(b) I dreamed that I was a German and that nobody realized it.

The problem with these examples is that, in both cases, the speaker apparently does not believe that the complement of the factive verb is true. In (11a), the pronoun *it* stands for the sentence 'that I had missed the train'. Since *regret* is a factive verb, the second clause of (11a) presupposes that the speaker has missed the train. However, this is just what is denied by the preceding counterfactual conditional. According to what we just said about factive verbs, (11a) ought to be self-contradictory. Similarly, (11b) ought to imply that the speaker believes that he is a German, even when he is not dreaming. Both of these predictions are clearly wrong. On the other hand, the examples in (12), which are very similar to those in (11) pose no problems at all.

(12) (a) If I had regretted that I missed the train, I would not have mentioned it.
(b) I dreamed that nobody realized that I was a German.

(12a) can be sincerely asserted only by someone who believes that he has missed the train; in (12b), the speaker must believe that he really is a German. The crucial difference between (11) and (12) is that, in (12a), the sentence with a factive predicate is the antecedent clause of a counterfactual conditional construction and, in (12b), it is the first sentence following the verb *dream*.

Morgan concludes from examples of this sort that the conditional *if*, the word *dream* and all similar verbs are to be regarded as 'world-creating' predicates. A sentence in the scope of a world-creating predicate is assumed to be true, not in the actual world, but in a 'possible world'.[7] A possible world receives its characterization in the usual left-to-right order of discourse. For instance, in (11b) the first sentence following the verb *dream*, 'I was a German', is understood to be a fact in the context of my dream world; therefore, it can stand as a presupposition for the following sentence, 'nobody realized that I was a German', which also is in the scope of *dream*. Similarly, in (11a) the antecedent clause of the conditional construction, 'I had missed the train', defines a possible world in which it may then also be true that I regret that fact.

This analysis explains the difference between the examples in (11) and (12). In (12b), the complement of *realize* has not been established as a fact of the dream world; therefore, it ought to be a fact in the actual world of the speaker. (12b) can only be said by someone who believes that he is a German. In (11b), the complement is introduced as a fact in a dream. It does not matter if the speaker does not believe it to be true in the actual world.

I don't intend to try to give any formal account of how possible worlds ought to be incorporated into a theory of language. I don't think that there is, at this point, much to be said about it beyond the kind of suggestive remarks that I have presented. This is an area where there is bound to be some exchange of ideas between linguists and modal logicians, who have traveled in possible worlds far more extensively than we have. But neither linguists nor philosophers have actually been thinking about sentences like those in (11) for very long.

1.3. DEGREES OF FACTIVITY. Another outstanding problem is that some of the factive verbs in (4) do not carry along the expected presupposition in all syntactic environments. For example, there is an unexplained difference between verbs like *regret* and *realize* in conditional clauses. Although both verbs are factive as far as simple assertions are concerned, *if*-clauses with *realize* as predicate do not presuppose the truth of the complement. Consider the difference between (13a) and (13b).

(13) (a) If I realize later that I have not told the truth, I will confess it to everyone.

(b) If I regret later that I have not told the truth, I will confess it to everyone.

It is obvious that (13a), with *realize* in the *if*-clause, does not presuppose that the speaker has not told the truth; it merely admits that there is such a possibility. On the other hand, one cannot utter (13b) without thereby conceding that one has not told the truth. Another ordinarily factive verb which loses its factivity under the same circumstances is the verb *find out*.

As far as I know, anomalies of this sort are still largely unexplored und poorly understood.

2. IMPLICATIVE VERBS. The next class of verbs I will discuss is illustrated by the examples in (14).

(14) IMPLICATIVE VERBS:

manage
bother
happen
see fit
get
care

have the $\left\{\begin{array}{l}\text{misfortune}\\ \text{foresight}\end{array}\right\}$
take (the) $\left\{\begin{array}{l}\text{time}\\ \text{opportunity}\\ \text{trouble}\end{array}\right\}$
condescend

If we just look at affirmative assertions, implicative verbs are very similar to factives. A sentence like the examples in (15) commits the speaker to the belief that the complement sentence is true.

(15) (a) Yesterday, Bill happened to break a window.
(b) To everyone's surprise, Sam managed to solve the problem.

(16) (a) Yesterday, Bill broke a window.
(b) To everyone's surprise, Sam solved the problem.

Asserting (15a) obligates the speaker to accept (16a) as true. The same goes for (15b) and (16b). But if we replace manage with a verb like decide, the same relation does not hold anymore. There is no such connection between (17) and (16b).

(17) To everyone's surprise, Sam decided to solve the problem.

However, notice that the adverbial modifiers of the main sentence, yesterday in (15a) and the phrase to everyone's surprise in (15b), by implication also seem to belong to the complement sentence. Another striking difference between factive and implicative verbs shows up in negative assertions. This can be observed by comparing the examples in (18) with those in (7). As you remember, in case of factives, negation in the main sentence has no effect on the assumed truth of the complement. But when a sentence with an implicative predicate is negated, it commits the speaker to the view that the complement is false. For instance, one cannot sincerely assert (18a) unless one believes (19a).

(18) (a) Sheila didn't bother to come.
(b) Max didn't have the foresight to stay away.

(19) (a) Sheila didn't come.
(b) Max didn't stay away.

It would be contradictory to say something like (20).

(20) *Sheila didn't bother to come, but she came nevertheless.

Similarly, (18b) implies (19b).

2.1. IMPLICATION. In saying that (18b) implies (19b), I am not using the term 'imply' in the sense of 'logically implies' or 'entails'. The relation is somewhat weaker, as indicated by the definition in (21).

(21) P implies Q iff
whenever P is asserted,
the speaker ought to believe that Q.

I believe this to be the same sense in which J. L. Austin (1962) has used the term. It is also closely related to B. C. Van Fraassen's (1968) notion of 'necessitation'.[8] Note that, for our weak sense of 'imply', the rule of inference known as 'Modus Tollens' does not apply. It is not required in (21) that asserting in $\sim$Q should, in turn, obligate the speaker to believe that $\sim$P. The reason why this point is worth making is that Modus Tollens is a valid argument form for the two other common senses of the term 'imply', 'materially implies' and 'logically implies', which we do not want to get mixed up with. Using the term in the sense of (21), we can say that (22a) implies (22b).

(22) (a) John managed to kiss Mary.
(b) John kissed Mary.

But it would be mistaken to conclude from this, by Modus Tollens, that the negation of (22b) implies the negation of (22a); in other words, that (23a) also implies (23b).

(23) (a) John didn't kiss Mary.
(b) John didn't manage to kiss Mary.

If you contemplate for a while the two sentences in (23), you will soon realize that one can perfectly well assert (23a) without committing oneself to the belief that (23b) is true.

The verb *manage* in (23b) carries along an extra assumption that is not shared by (23a). It would be appropriate to use (23b) only if John had actually made an unsuccessful attempt to kiss Mary. Therefore, these two sentences are not logically equivalent; the implication only holds in one direction, from (23b) to (23a) and from (22a) to (22b).

2.2. MEANING POSTULATES. Let us now consider the problem how these facts about implicative verbs ought to be accounted for. One might, for example, propose that the semantic representation of (15a) actually contains the implied sentence, (16a), as a subpart. If one is a generative semanticist, one might even assume that (15a) be transformationally derived from some structure that properly includes the underlying structure of (16a). Under this proposal, there would be no distinction between the semantic representation of a single sentence and the set of inference derivable from it; the two notions would be equivalent.[9] This is not the approach that I have chosen. Instead, I assume that the implied sentence is not included in the underlying representation of its antecedent but is to be derived from it by means of meaning postulates and general rules of inference.

I have proposed (Karttunen 1970a) that the facts about implicative verbs be accounted for in the following manner. What all verbs such as *manage*, *bother*, etc. have in common is that they are understood to represent some necessary and sufficient condition which alone determines whether the event described in the complement takes place. They all have the same two meaning postulates associated with them.[10] Using *v* for any arbitrary implicative verb and *S* for its complement, we can represent these two meaning postulates roughly as in (24).

(24) (a) $v(S) \supset S$ 'v(S) is a sufficient condition for S'

(b) $\sim v(S) \supset \sim S$ 'v(S) is a necessary condition for S'

What actually constitutes this decisive condition depends on the particular implicative verb. It may consist of making a certain effort, as in *bother*, showing enough skill and ingenuity, as in *manage*, or it may be a matter of chance, as in *happen*. A sentence with one of these verbs as predicate can be looked upon as a statement about whether this decisive condition is fulfilled, and under what spatial and temporal circumstances this is the case. From an affirmative assertion, we can then infer that the complement is true; from a negative assertion that the complement is false. The rule of inference I am assuming here is, of course, the familiar Modus Ponens, which is illustrated in (25).

(25) MODUS PONENS:

$$\text{(a)}\quad \begin{array}{c} P \supset Q \\ P \\ \hline \therefore Q \end{array} \qquad\qquad \text{(b)}\quad \begin{array}{c} \sim P \supset \sim Q \\ \sim P \\ \hline \therefore \sim Q \end{array}$$

For example, note that although all the sentences in (26a) assume a different kind of decisive condition for the truth of the complement, they all assert that, yesterday, this condition was not fulfilled.

(26) (a) Yesterday, John did not $\left\{\begin{array}{l}\text{happen}\\ \text{manage}\\ \text{bother}\\ \text{see fit}\end{array}\right\}$ to kiss Mary.

(b) Yesterday, John did not kiss Mary.

Therefore, (26b) can be derived in all cases as a legitimate inference in the manner illustrated in (25b) above.

2.3. NEGATIVE IMPLICATIVES. Next I would like to point out a group of verbs that are in every other respect like the implicative verbs in (14) except that they work the opposite way. A short list of these negative implicatives is given in (27).

(27) NEGATIVE IMPLICATIVES:
forget (to)
fail
neglect
decline
avoid
refrain

An affirmative assertion with one of these verbs as predicate implies the negation of the complement. For instance, (28a) implies (28b).

(28) (a) John avoided getting caught in the traffic.
(b) John didn't get caught in the traffic.

On the other hand, a negative assertion results in a positive implication, just as we would expect on the basis of the Law of Double Negation. Thus (29a) implies (29b).

(29) (a) John didn't avoid getting caught in the traffic.
(b) John got caught in the traffic.

There are in principle two ways to account for these facts in our analysis. One way is to say that we have a separate pair of meaning postulates for negative implicative verbs. This set would be the pair given in (30).

(30) (a) $v(S) \supset \sim S$ 'v(S) is a sufficient condition for $\sim S$'
(b) $\sim v(S) \supset S$ 'v(S) is a necessary condition for $\sim S$'

The other possibility is to assume that negative implicatives in fact contain negation in their underlying syntactic structure and that there is a process of lexical insertion that can replace some ordinary implicative verb and the preceding negation marker with one of the verbs in this special class. For instance, there would be rules such as (31), which says that the verb fail, in one of its senses, is equivalent to not succeed. This equivalency may then be interpreted as permission to substitute fail for not succeed in some underlying syntactic structure.

(31) fail ≡ not succeed

One immediate advantage of such an analysis is that only one set of meaning postulates is needed, namely the pair in (24). On the other hand, there is the apparent problem that most of the verbs in (27) do not have any implicative positive counterpart. What, for instance, would be the positive counterpart of avoid?

At this time, I do not know of any decisive argument for choosing between the two alternative analyses that are available for negative implicatives.

2.4. SPECIAL CASES. In addition to the verbs listed in (14) and (27), there are of course many other implicative verbs. After one becomes aware of their existence, they are not hard to catch. There are some that are especially interesting. For instance, the words true and false, at least in their everyday sense, are implicative. They would, in fact, be the best example to use, if one wanted to argue that negative implicatives are to be defined in terms of positive ones. Nobody but a three-valued logician would refuse to accept the word false as the equivalent of not true. Another implicative word is the noun fact, which is not factive, as one might expect from the name. For that reason, it may be appropriate at this point to sound a warning and say that the verb imply, in turn, is not implicative. On one hand, it is a factive verb; on the other hand, it may also be a member of another category that we have not discussed yet: the if-verbs.

3. *IF*-VERBS AND *ONLY-IF* VERBS. The next two classes of verbs also give rise to implicative relations, although in a less perfect fashion than implicative verbs proper. What is common to both of these types is a kind of asymmetry between negative and affirmative sentences, so that the implication holds only in one of them. It appears to me that these verbs are associated with only one of the two meaning postulates in (24). Verbs of one group express a sufficient condition for the truth of the complement. For that reason - and for the sake of brevity - I refer to them as '*if*-verbs'. Verbs in the other group express a necessary condition; they are the '*only-if*-verbs'. Later on, I will sometimes refer to *if*-verbs and *only-if*-verbs jointly as 'one-way implicatives' in order to distinguish them from 'two-way implicatives' discussed above, that is, from verbs which yield an implication both in negative and in affirmative assertions.

3.1. *IF*-VERBS. The set of *if*-verbs includes those in (32).

(32) *IF*-VERBS:

$$\begin{array}{ll} \text{cause} & \text{make sure} \\ \text{make} & \text{bring about} \\ \text{have} & \text{see to it} \\ \text{force} & \end{array} \quad ? \begin{pmatrix} \text{certain} & \text{mean} \\ & \text{imply} \\ & \text{indicate} \\ \text{prove} & \text{show} \end{pmatrix}$$

If one of these verbs appears as the main verb of an affirmative assertion, the complement is implied to be true. For instance, consider the examples in (33a).

$$\text{(33)}\quad \text{(a)}\quad \text{John} \begin{Bmatrix} \text{forced} \\ \text{made} \\ \text{caused} \end{Bmatrix} \text{Mary to stay home.}$$

$$\text{(b)}\quad \text{John} \begin{Bmatrix} \text{ordered} \\ \text{asked} \\ \text{advised} \end{Bmatrix} \text{Mary to stay home.}$$

In all of the a-sentences, the speaker is committed to the belief that Mary stayed home. It would not be honest to assert any of the sentences in (33a) if one thought otherwise. This fact distinguishes the verbs in (33a) from such syntactically very similar verbs as those in (33b). It is clear that none of the sentences in (33b) has a definite implication one way or the other.

On the other hand, in negative assertions, the difference between _if_-verbs and those in (33b) disappears entirely. In (34), _force_ and _order_ are just alike; both are equally non-committal with respect to the complement sentence.

(34) (a) John did not force Mary to stay home.
(b) John did not order Mary to stay home.
(c) ... $\left\{\begin{array}{l}\text{and she didn't.}\\ \text{but she stayed home anyway.}\end{array}\right\}$

As you can easily observe, (34c) is an acceptable continuation for both of the preceding sentences. This fact indicates that, although the verb _force_ expresses a sufficient condition for the truth of the complement sentence, it does not express a necessary condition. We can only assign to it the first of the two meaning postulates in (24), namely

(24a) $v(S) \supset S$ 'v(S) is a sufficient condition for S'.

Thus far I have only discussed _if_-verbs which take infinitive complements. But in general there appears to be no connection between the semantic properties of a verb and the syntactic type of complement clause it takes. Just as there are factive verbs with infinitive complements, such as _wise_ and _proud_, there are also _if_-verbs which take _that_-complements; for example, _bring about_, _see to it_, and _make sure_. That these verbs really are _if_-verbs and not factives can be shown by pointing out that (35) can be asked felicitously by someone who does not know whether Mary got what she wanted.

(35) Did Bill see to it that Mary got what she wanted?

Furthermore, an affirmative answer commits you to the claim that Mary actually got what she wanted; a negative answer is non-committal in this respect.

It is interesting to notice that all the clear _if_-verbs seem to be, in some intuitive sense, causative verbs. It would be very interesting to find some clear cases of non-causative _if_-verbs, but all the likely candidates that I have come across appear to involve some additional complications. For example, consider the word _certain_. There is no doubt that _certain_ is an _if_-verb in constructions like (36a).

(36) (a) It is certain that Sheila left with Max.
(b) Bill is certain that Sheila left with Max.

Surely, it would be dishonest to say (36a) if you did not believe that Sheila left with Max. But it is also clear that _certain_ is not an _if_-verb in (36b). It seems likely that, in addition to the complement clause, the verb _certain_ always involves another underlying noun phrase, in Fillmore's terms, an 'experiencer '.[11] This noun phrase may remain unexpressed if it is identical with the speaker, as in (36a). The verb _certain_ does not count as an _if_-verb unless the experiencer and the speaker are the same person.

The same problem shows up in verbs like _mean_ and _imply_, as you can observe from the examples in (37).

(37) (a) That the grass is wet implies that it has been raining.
(b) For Bill, it means that somebody has watered the lawn.

In (37a), the speaker commits himself to the view that is has been raining. But (37b), where the experiencer is not identical with the speaker, is non-committal with regard to the complement. Another fact about these verbs is that, as far as the subject complement is concerned, they are factive. (37a) and (37b) both presuppose that the grass is wet. Because of these complications, it is not clear whether these verbs should really be regarded as if-verbs at all.

Another interesting case is the verb prove. Unlike the verbs just mentioned, prove meets the criteria for if-verbs no matter who the 'experiencer' is. All of the examples in (38) imply the truth of the complement.

(38) (a) Bill proved to me that Max was a liar.
(b) Bill proved to Sally that Max was a liar.
(c) That there is no money in the bag proves that Max is a liar.

On the other hand, the corresponding negative assertions are non-committal.

(39) That there is no money in the bag doesn't prove that Max is a liar; perhaps he is, perhaps he isn't.

As far as these data are concerned, there is no reason not to consider prove as an if-verb.

However, it is also possible to account for just the same facts by a more complex analysis of prove. Let us assume that prove is associated with the meaning postulate in (40), in which the consequent consists of a causative sentence with a factive complement.

(40) $\text{prove}(x, y, S) \supset \text{cause}(x, \text{know}(y, S))$

The fact that all the examples in (38) imply their complement can now be explained by the combined effect of cause and know. For example, given the meaning postulate in (40), (38b) implies (41a), which in turn implies (41b). The latter sentence has a factive predicate; therefore, it presupposes (41c), which is the desired inference.

(41) (a) Bill caused Sally to know that Max was a liar.
(b) Sally knew that Max was a liar.
(c) Max was a liar.

On the other hand, the fact that (39) is non-committal with respect to (41c) is explained by the fact that, since _cause_ yields no implication in a negative assertion, one cannot infer from (39) either that I know Max to be a liar or that I don't know that he is.

The same type of analysis can also be applied to verbs like _indicate_, _show_, etc. Assuming that such verbs are analyzed roughly as in (42), we can explain some of the puzzling facts mentioned earlier.

(42) indicate(x, y, S) $\supset$ cause(x, believe(y, S))

For example, we have an explanation for the fact that only the first of the following two examples commits the speaker to the complement sentence.

(43) (a) That there is no money in the bag indicates to me that Max is a liar. (*But I don't think that he is.)
(b) That there is no money in the bag indicates to Sally that Max is a liar. (But I don't think that he is.)

The fact that, in (43), the identity of the 'experiencer' determines whether or not the implication holds can be attributed to the fact that the complement of _cause_ in (42) contains a non-factive verb. For this reason, (43b) only implies that Sally believes Max to be a liar; it is non-committal as far as the speaker is concerned.

3.11. NEGATIVE _IF_-VERBS. There is also a class of negative _if_-verbs, which includes the verbs listed in (44).

(44) NEGATIVE *IF*-VERBS: prevent discourage
dissuade
keep (from)

Like their positive classmates, negative *if*-verbs carry along a commitment with regard to the complement in affirmative assertions. The difference is that the complement is implied to be false. For example, (45a) definitely implies that Mary did not leave.

(45) (a) John prevented Mary from leaving.
(b) ...*but she left anyway.

On the other hand, a negative assertion such as (46a) is non-committal. It is compatible with either one of the two continuations in (46b). It is this fact which distinguishes *prevent* from *avoid* and other such two-way implicatives listed in (27). They are committal even in negative assertions.

(46) (a) John didn't prevent Mary from leaving.
(b) ... $\left\{\begin{array}{l}\text{and she left.}\\ \text{but she chose not to leave.}\end{array}\right\}$

Negative *if*-verbs bring up the same problem as negative implicatives. In principle, there are three ways to account for their negative properties. One way is to postulate for them the first of the two meaning rules in (30):

(30a) $v(S) \supset \sim S$ 'v(S) is a sufficient condition for $\sim S$'.

The other possibility is by way of lexical insertion rules that replace some piece of underlying syntactic structure including a negation marker by one of the verbs in (44). This alternative has been proposed by George Lakoff (1969). It is easy to see, for instance, that we could account for the negative implication of *discourage* by defining it as in (47a).

(47) (a) discourage ≡ cause not to have courage
(b) keep ≡ cause not to be able

On the other hand, it is a little harder to see what the underlying structure of keep might be. (47b) is an educated guess. Of course, it remains to be shown that the logical properties of keep really correspond to those of the complex predicate cause not to be able. That this is really the case will hopefully become clear later on when we discuss the semantics of be able and other 'only-if-verbs'.

The third possible way to account for negative if-verbs is the same that was already suggested in connection with prove and indicate: setting up new meaning postulates. Obviously, these meaning postulates would be similar to Lakoff's lexical insertion rules. Instead of having a rule for replacing the structure cause not to be able with the single lexical item keep, one would set up a meaning postulate such as (47b*), which allows one to derive the former from the latter as a logical inference.

(47b*) $\text{keep}(x, y, S) \supset \text{cause}\ (x, \sim\text{able}(y, S))$

It is doubtful whether there is any conclusive argument for choosing between the last two alternatives. However, note that (47b*) makes a weaker claim than its predecessor. Unlike a Lakoff-type insertion rule, it is not open to objections which are based on the claim that the transformationally inserted lexical item is not really synonymous with its supposed paraphrase.

Instead of trying to settle the issue here, I will simply assume that negative if-verbs are associated with the meaning postulate (30a), which is also shared by avoid and other similar two-way implicatives.

3.12. OTHER IMPLICIT CAUSATIVES. One interesting side result from the study of if-verbs is that it lends some new support to the so-called 'causative analysis' of verbs like kill and break. James D. McCawley (1969) and others have proposed that such verbs should not be treated as unanalyzed lexical items in underlying syntactic representations. Instead, they should be inserted transformationally by a rule that replaces a subtree in which *cause* is the topmost predicate. According to this view, the underlying structure of *kill* is roughly as in (48).

(48) kill ≡ cause to become not alive

Since *cause* is an *if*-verb, it follows from this analysis that *kill* should also belong to this semantic category. As the following example shows, this prediction seems to be in agreement with out intuitive judgements. An affirmative assertion with *kill* as predicate implies that the person referred to by the object NP dies (i.e. 'becomes not alive'). Thus (49a) implies (49b).

(49) (a) John killed Harry.
(b) Harry died.
(c) John didn't kill Harry.

A negative assertion, such as (49c), is non-committal in this respect. (49c) is compatible with the belief that Harry is still alive, or with the belief that Harry is dead but somebody else killed him. The causative analysis of *kill*, with no further assumptions, correctly explains the semantic relations between the three sentences in (49). Again, if it should become impossible to maintain the view that *kill* is introduced transformationally, there will have to be a corresponding meaning postulate, such as (48*).

(48*) $\mathrm{kill}(x, y) \supset \mathrm{cause}(x, \mathrm{become}(\sim\mathrm{alive}(y)))$

3.2. *ONLY IF-VERBS*. The second major group of one-way implicatives that deserves our attention are the *only-if*-verbs, of which a sample is given in (50).

(50) *ONLY-IF*-VERBS:

can
able
possible
be in the position
have (the) {time, opportunity, patience, courage}

If one of these verbs appears as the main verb of a negative assertion, the complement is implied to be false, as shown by the examples in (51).

(51) (a) Sebastian did not have an opportunity to leave the country.
(b) Sebastian was not able to leave the country.
(c) ...*but he left anyway.

In (51) and (51b), the speaker is committed to the view that Sebastian did not leave. It would be contradictory to continue either sentence with (51c). This fact indicates that the verbs in (50) express a necessary condition for the truth of the complement. That is, they are associated with the second meaning postulate in (24), namely

(24b) $\sim v(S) \supset \sim S$ 'v(S) is a necessary condition for S'.

Given this meaning postulate, we can infer from a negative assertion like (51a) and (51b) that the complement is implied to be false.

In the corresponding affirmative assertions, however, there is no definite implication one way or the other. The two examples in (52) are both compatible with the continuation in (46c).

(52) (a) Sebastian had an opportunity to leave the country.
(b) Sebastian was able to leave the country.
(c) ... but he chose not to do so.

Therefore, the verbs in (50) are not two-way implicatives; they do not express a sufficient condition for the truth of the complement.

It is perhaps worth pointing out that there are at least three semantically different groups of predicates that all appear in the same surface construction, *have the X (to)*. Some of them are full two-way implicatives like *have the foresight* and *have the misfortune*, which we encountered in (14); those in (50) are only one-way implicatives. The third class consists of predicates which do not carry along any implication at all with respect to the complement sentence. A sample of them is given in (53).

(53)

$$\text{have (the)} \begin{Bmatrix}\text{right}\\ \text{authority}\\ \text{permission}\\ \text{orders}\end{Bmatrix}$$

It is easy to see that a negative assertion with any of these verbs as predicate is non-committal. Unlike the similar examples in (51), (54) leaves open the possibility that Sebastian may have left anyway.

(54) Sebastian did not have a permission to leave the country.

3.21. NEGATIVE *ONLY-IF*-VERBS. Since there are both negative two-way implicatives and negative *if*-verbs, one expects to find some negative *only-if*-verbs as well. A verb of this sort would be like *be able* and other positive *only-if*-verbs in the respect that it would yield a definite implication only in negative assertions. However, the implication must be of the opposite kind, that is, a positive implication. These verbs would be associated with the second meaning postulate in (30), namely

(30b) $\sim v(S) \supset S$ 'v(S) is a necessary condition for $\sim$S'.

On the other hand, affirmative assertions with such a verb as predicate should be non-committal.

The class of verbs which have the desired properties appears very small. The only verb I know of which certainly is a negative *only-if*-verb is the word *hesitate*.[12] Consider the following example.

(55) (a) Bill did no hesitate to call him a liar.
(b) Bill called him a liar.

Whoever asserts (55a) commits himself to (55b). However, the corresponding affirmative assertion, (56a), is non-committal. It is compatible with either one of the two continuations in (56b).

(56) (a) Bill hesitated to call him a liar.
(b) ... {Therefore, he didn't say anything.
but his conscience forced him to do so.}

That is, *hesitate* is not a two-way implicative like *avoid*.

There is no obvious reason why *hesitate* should be the only verb of its kind, but thus far I have not found any other negative *only-if*-verbs.

Note that *hesitate* and *prevent*, which is a negative *if*-verb, both share one of the two meaning postulates in (30), which jointly account for the semantics of two-way implicatives such as *avoid*. These three verbs stand in the same relation to each other as their corresponding positive counterparts *be able*, *cause*, and *manage*, which share the meaning postulates in (24). As we mentioned above, it may be possible to eliminate the class of negative *if*-verbs, such as *prevent*, with the help of their positive classmates by regarding them as replacements for structures like *cause not to be able*. If this method were applicable to all negative implicatives, there would be no need for the second pair of meaning postulates in (30). However, it is doubtful whether verbs like *avoid* and *hesitate* can be lexically decomposed in a similar manner. Therefore, I will assume for the time being that the two sets of meaning postulates, (24) and (30), are both needed.

4. APPLICATIONS. I have now introduced six categories of implicative verbs: two types of two-way implicatives and four types of one-way implicatives. Most of the examples thus far have been very simple sentences with no more than one level of embedding. It is now time to look at some more complicated cases, in which verbs of different types alternate with negation in the same complex sentence. We should check that the semantic relations predicted by our analysis continue to agree with our intuitive judgements. Consider first the example in (57).

(57) (a) Bill saw to it that the dog did not have an opportunity to run away.
(b) The dog did not have an opportunity to run away.
(c) The dog did not run away.

Since (57a) is an affirmative assertion and has an if-verb as predicate, it implies (57b). This is a negative sentence with an only-if-predicate; therefore, it implies the negation of its own complement, which is (57c). Thus there is a chain of implications from (57a) to (57c). Since the notion 'implies' obviously is a transitive relation, (57a) should imply that the dog did not run away. Now, look at another configuration of the same verbs in (58).

(58) (a) Bill had an opportunity to see to it that the dog did not run away.
(b) Bill saw to it that the dog did not run away.
(c) The dog did not run away.

Since have an opportunity is an only-if-predicate, although (58a) is an affirmative assertion, it does not imply the truth of its complement sentence, which is (58b). If (58b) were itself implied to be true, it would in turn imply (58c). But since (58b) is not implied by (58a), there is no chain of implications that would link (58a) with its lowest embedded sentence. Therefore, (58a) should not commit the speaker to any view whatever about the dog. It seems clear that the predicted semantic relations in these and other similar cases turn out to match our intuitive judgements.

Incidentally, note that the example in (59) carries along the same implication as (57a).

(59) John prevented the dog from running away.

Note also that the negations of (57a) and (59) are equally non-committal with regard to the truth of the complement.

Negative if-verbs, such as prevent, are in this respect equivalent to the configuration: if-verb ... negation ... only-if-verb. It is this fact which makes it possible to propose that they be introduced by a transformation.

As a final example, consider the sentence in (60).

(60) Bill did not have the foresight not to force Mary to prevent Sheila from having an opportunity to try that new detergent.

The question is whether (60) is non-committal with respect to the truth of its lowest embedded clause or whether one is justified in inferring from it that Sheila either tried or did not try the new detergent. Although most people at first do not feel sure one way or the other, it does not take long to discover that (60) must mean that she did not try it. We can show this formally in the following way. Let us represent (60) schematically as (61).[13]

(61) $\sim V_1 (\sim V_2 (V_3 (V_4 (S))))$

Where V_1 = have the foresight [two-way implicative]

V_2 = force [if-verb]

V_3 = prevent [negative if-verb]

V_4 = have an opportunity [only-if-verb]

S = Sheila tried that new detergent.

Assuming that the verbs in question have the semantic properties that we have assigned to them, it can be shown that (61) yields the desired inference. In the following, the number on the right of each line refers to the meaning postulate that was used in deriving that line from the preceding one.

(62) (a) $\sim V_1 (\sim V_2 (V_3 (V_4 (S))))$ [= (61)]

(b) $\sim\sim V_2 (V_3 (V_4 (S)))$ - (24b)

(c) $V_2 (V_3 (V_4 (S)))$ - Law of Double Negation

(d) $V_3 (V_4 (S))$ - (24a)

(e) $\sim V_4$ (S) - (30a)

(f) $\sim S$ - (24b)

The last line of (62) indicates that, according to the proposed analysis, (60) implies (63).

(63) Sheila did not try that new detergent.

The present example may well be too complicated for some speakers to understand. However, it seems that, as far as people have any intuitions at all about its meaning, their judgements support the proposed analysis.

5. INVITED INFERENCES. There are certain important facts that have not yet been accounted for. Consider the example in (64a).

(64) (a) John's wooden leg didn't keep him from dancing with Mary.
(b) John danced with Mary.

If one reads (64a) in isolation without thinking too much about it, one is very likely to get the impression that John danced with Mary, in spite of his wooden leg. However, a more careful analysis of (64a) shows immediately that this sentence does not imply (64b). As a negative if-verb, keep (from) should yield an inference only in affirmative assertions. Since (64a) is a negative assertion, it should be non-committal, as far as (64b) is concerned. This is certainly not a false prediction, as shown by the fact that (64a) can, without any contradiction, be embedded into a context where it is made clear that John did not dance with Mary. For example, (64a) can be expanded to (65).

(65) John's wooden leg didn't keep him from dancing with Mary, but her husband did.

Nevertheless, in the absence of any contrary evidence, (64a) seems to suggest that John danced with Mary.

The following example is similar. Since force is an if-verb and it occurs here in a negative assertion, (66a) should be non-committal with respect to (66b).

(66) (a) Bill did not force Mary to change her mind.
(b) Mary did not change her mind.

However, it seems that there is a temptation to conclude (66b) from (66a) if no further information is given.

The same phenomenon shows up with only-if-verbs. If there is no particular reason to believe otherwise, most people will take (67a) to mean that John in fact left early.

(67) (a) John was able to leave early.
(b) John left early.

Again, (67a) should be non-committal. Since be able is classified as an only-if-verb, it yields an implication only in a negative assertion. Why should it be that, although (67a) does not logically imply (67b), it nevertheless strongly suggests that (67b) is true? Here, as in the two preceding examples, a one-way implicative predicate invites one to draw a conclusion which would logically follow only from a two-way implicative verb. That is, in concluding (67b) from (67a) one interprets be able as if it were a verb like manage.

It is very likely that this problem is another manifestation of a principle which Michael Geis and Arnold Zwicky (1970) have discussed in connection with conditional sentences. As Geis and Zwicky point out, there is a natural tendency in the human mind to perfect conditionals to biconditionals. Students in an elementary logic course often propose that examples such as (68) are to be formalized as biconditionals rather than conditionals.

(68) If you mow the lawn, I'll give you five dollars.

Thus, most people feel that the appropriate logical form of statements like (68) is the conjunction of (69a) and (69b).

(69) (a) $S_1 \supset S_2$
(b) $\sim S_1 \supset \sim S_2$

This is not quite right since (69a) alone is enough. However, it is clear that in a great majority of cases where a conditional like (68) is uttered, the corresponding statement of the form (69b) is also tacitly assumed. In natural language, (68) suggests rather strongly that, if you don't mow the lawn, I won't pay you five dollars. What would be the point in stating a condition which was not a necessary condition for the truth of the consequent? According to the principle proposed by Geis and Zwicky, any assertion of the form (69a) suggests, or "invites the inference", that the corresponding statement of the form (69b) is also true. However, this is only an "invited inference" and the speaker may indicate that it does not hold without thereby contradicting himself. This is the case in (70).

(70) If you mow my lawn, I'll give you five dollars, but
I'll give you five dollars even if you don't.

The only thing that is odd about (70) is that it makes one wonder why anyone would bother to set a condition which is not a necessary one. (70) may be pointless but it is not contradictory.

Similarly, we can say that, although an *if*-verb, such as *force* in the example (66a), strictly speaking is associated only with the meaning postulate (24a) $v(S) \supset S$, it also "invites" the corresponding negative meaning postulate (24b) $\sim v(S) \supset \sim S$. This explains why (66a) suggests (66b), although it does not actually imply (66b). On the other hand, an *only-if-*verb like *be able*, which is associated with the meaning postulate (24b) $\sim v(S) \supset \sim S$, "invites" (24a) $v(S) \supset S$. This is the reason for the tempation to conclude (67b) from (67a). Something like the Geis-Zwicky principle is clearly involved in the general tendency to understand one-way implicatives as full two-way implicatives, unless the context makes it necessary to interpret them more strictly.[14]

6. SUMMARY. The following chart is a review of the semantic classes of verbs which have been discussed in this paper. The chart indicates under what circumstances a main sentence implies the complement or its negation in each of the seven categories. The '+' sign is used when a sentence is to be regarded as true; '-' is a symbol for a false sentence. The '+/-' sign means that a sentence may either be regarded as true or regarded as false. The variable 'α' may take either + or - as its value. It is used to indicate that the complement has the same truth value as the main sentence. A complement which has the opposite truth value with respect to the main sentence is marked with '-α'.

(72)

CLASS	MAIN SENTENCE	COMPLEMENT SENTENCE	EXAMPLE
Factive	+/-	+	regret
Implicative	α	α	manage
Negative Implicative	α	-α	avoid
If-Verb	+	+	cause
Negative If-Verb	+	-	prevent
Only-If-Verb	-	-	be able
Negative Only-If-Verb	-	+	hesitate

From the first column of features one can see under what conditions the main sentence carries along a commitment with respect to the truth of falsity of its complement. For example, it shows that a sentence with a negative if-verb as predicate just in case it is to be regarded as true. The second feature column indicates what is implied. In the case of negative if-verb the complement is implied to be false. On the other hand, sentences with an only-if-verb as predicate carry along an implication with respect to the complement only if they are to be regarded as false. Full two-way implicatives such as manage and avoid, yield an implication both in affirmative and negative assertions and the implication is affirmative or negative depending on the main sentence and the type of the verb.

Finally, the next chart gives a sample verb from each category of implicative verbs and the meaning postulate(s) it is associated with.

(72)

(24)	(a)	$v(S) \supset S$	cause	manage
	(b)	$\sim v(S) \supset \sim S$	be able	
(30)	(a)	$v(S) \supset \sim S$	prevent	avoid
	(b)	$\sim v(S) \supset S$	hesitate	

It is evident that logical relations between main sentences and their complements are of great significance in any system of automatic data processing that depends on natural language. For this reason, the systematic study of such relations, of which this paper is an example, will certainly have a great practical value, in addition to what it may contribute to the theory of the semantics of natural languages. It also seems to be the case that logical relations are also involved in a number of problems that have sometimes been regarded as purely syntactic. Two well-known examples of such phenomena are the constraints on coreference (Karttunen 1969) and the problem of polarity-sensitive lexical items (Baker 1970).

FOOTNOTES

[1] This report is an extended version of a paper that was originally written under the auspices of the 1970 MSSB Advanced Research Seminar in Mathematical Linguistics and presented at the Annual Meeting of the Association for Computational Linguistics, July 23, 1970, in Columbus, Ohio.

[2] The following are some of the relevant papers: Karttunen 1970a and 1970b, Kiparsky 1968, Lakoff 1969 and 1970, Morgan 1969.

[3] A summary of various arguments against Rosenbaum is presented in the Final Report of the UCLA Syntax Project (UESP Vol. II).

[4] In their paper, the Kiparskys distinguished between 'semantically factive' and 'syntactically factive'. The reason for this distinction was that a few words, e.g. know and realize, which would otherwise qualify failed to meet some of the syntactic criteria the authors had established for factivity. I will use the term 'factive' as equivalent to their 'semantically factive'. Also, I do not see any reason to assume, as they did, that surface complements of factive verbs are commanded by the noun fact in the deep structure.

[5] This particular definition is due to Van Fraassen 1968.

[6] Currently, there are at least three different locutions in use; sometimes they can all be found in the same article:

(i) [in uttering the sentence X] 'the speaker presupposes that ...'

(ii) 'the verb X presupposes that ...'

(iii) 'the sentence X presupposes that ...'

Actually, (i) and (ii) seem to be some sort of shorthand formulas for longer expressions that involve (iii). For example, (ii) is generally intended to mean something like 'any sentence with the verb X as predicate presupposes that ...'. It need not be the case that there is really a genuine confusion in the mind of the users about whether 'presupposition' is an act by the speaker, a relation between a verb and its complement, or a relation between two propositions. However, the indiscriminate usage of (i-iii) is likely to breed such confusion in the minds of others.

[7] The notion of 'possible world' is borrowed from modal logicians; e.g. see Hintikka 1967.

[8] Van Fraassen 1968 gives the following definition for 'necessitation':

> P necessitates Q if and only if, whenever P is true, Q is also true.

This relation has also been called 'semantic entailment'.

[9] These are equivalent concepts for some writers. For example, Irene Bellert (1969) explicitly defines the semantic representation of an utterance as the set of consequences which can be derived from it.

[10] The notion of 'meaning postulate' comes from Rudolf Carnap (1947). As he uses the term, a meaning postulate is a sentence of a formalized language which expresses a relation that holds between some primitive predicates of that language. For example, if the analyticity of the English sentence "If Sheila is a spinster, then she is not married" is to be preserved in translating it into a formal language, the language has to include the statement

$$(x)\ \text{spinster}(x) \supset \sim\text{married}(x)$$

as a meaning postulate.

In our case, we need meaning postulates to account for logical relations between main sentences and their complements.

[11] Verbs of this kind were discussed by Charles Fillmore in his lectures at the 1970 Linguistic Institute. The 'Experiencer' case together with another new case, 'Goal', have replaced what Fillmore used to call the 'Dative' case in earlier presentations of his case theory.

[12] The verb be afraid comes close to being a negative only-if-verb. Although one can argue that

(i) The princess wasn't afraid to kiss the frog.

does not actually imply that she kissed him, the suggestion that she did is quite strong unless something is said to indicate otherwise.

Another possible candidate is help in the construction can't help ...ing. If help were a negative only-if-verb, (ii) should imply (iii) and (iv) ought to be non-committal.

(ii) The frog couldn't help feeling happy.
(iii) The frog felt happy.
(iv) *The frog helped feeling happy.

Indeed, (ii) implies (iii). But help could also be a two-way implicative like avoid, in which case (iv) ought to imply that the frog didn't feel happy. Since (iv) is ungrammatical, it is hard to decide one way or the other.

[13] For the sake of simplicity, I treat all the verbs in (60) as if they were one-place predicates. As throughout this paper, I

also ignore the problem how the correct tense is assigned to implied sentences.

[14] This observation may also explain the alternation between *and* and *but* in certain cases. For example, consider the example (46a) with its two alternative continuations. Since *prevent* is a negative *if*-verb, (46a) suggests, but does not imply, that Mary left. We get *but* instead of *and* as the conjunctive particle if the conjoined sentence cancels the suggested inference.

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The Feasibility of High Quality Machine Translation

by

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THE FEASIBILITY OF HIGH QUALITY MACHINE TRANSLATION

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This brief memorandum is intended as a contribution to the project entitled "Theoretical Study Effort of High Quality Translation" being conducted at the Linguistics Research Center (LRC) under Contract F30602-70-C-0129. It is a simple restatement of views expressed during formal and informal discussions in which I have taken part at LRC in September and October 1970.

The aim of the project is to evaluate the developments that have taken place in linguistics and computer science in the last five or six years in relation to the feasibility of fully automatic high quality translation (FAHQT). Not having any specialist knowledge of computer hardware or software, I will confine myself to the linguistic aspects of the problem. I would, however, point out that the doubts expressed by such scholars as Bar-Hillel about the feasibility of FAHQT in the early part of the 1960's had very little to do with the speed of operation and capacity of computers that were available at the time. Their critical attitude towards the feasibility of FAHQT was determined far more by their increased appreciation of the theoretical complexity (and perhaps ultimate impossibility) of specifying algorithmic procedures for syntactic analysis and for the resolution of semantic ambiguities. Advances made in computer capabilities over the last decade would not therefore seem to be very relevant to an appraisal of the validity of their statements in the light of present knowledge (Advances in computer capabilities may of course be relevant to the pursuit of some less ambitious system of MT, to which I refer below.)

Let me begin by making the obvious point that "feasibility" presupposes "possibility." Is FAHQT even possible? This question cannot be answered without first deciding what "high quality" means in this context. Not even the most competent human translators can be relied upon to produce a "perfect" translation of any text submitted to them. Moreover, it is arguable that in certain styles or certain subjects of discourse "perfect" translation is impossible in principle. A fortiori, it is not feasible. The point I have just made would be accepted by some linguists and rejected by others; and I am conscious of the fact that my attitude is here in conflict with that of many semanticists, whose work over the last few years has been inspired by their commitment to the possibility of describing the semantic structure of all languages in terms of a set of universal semantic "features" (or "atomic concepts"). I give it as my opinion that, at the present time, the predispositions of certain scholars in favour of universal semantics are methodological or philosophical in character, and cannot yet be justified by any convincing appeal to empirical evidence. The fact that many influential linguists have adopted the "universalist" position in recent years is, I believe, irrelevant to our reappraisal of the feasibility of MT.

Bar-Hillel has recently drawn attention to the necessity of making the notion of "quality" relative to the purpose for which a translation is intended. I would endorse his pragmatic attitude to this question; and I would make two further comments in the same pragmatic spirit. One kind of "quality" (in a translation system, rather than in a translation) might relate to the range, in style and content, of the material for which the system is designed. It would be my assumption that any MT system that is really intended to be at all viable will deliberately restrict the range of the input that it will accept. It may be impossible to specify any lexical item (or any particular sense of a lexical item) or any construction such that one can be certain it will never occur in "scientific English" (or whatever the source language is). But some restriction of style and content is essential.

A second kind of "quality" might relate to the incidence of failures to translate or of mistranslations. The difficulty of foreseeing all possible cases of this kind and of programming for the recognition of contextual cues to the resolution of ambiguity (granted that the text will always contain determinable cues) was one of the principal difficulties referred to by Bar-Hillel and other critics of FAHQT in the early 1960's. None of the recent work in syntax and semantics would lead me to believe that the prospects for FAHQT have in this respect improved. At the present time, it would seem to be impossible to design an MT system that is absolutely "fail-safe"; and it may very well be the case that this goal will never be achieved. Just how serious a problem this is in practice can hardly be decided on theoretical grounds. It may be that texts of the kind that are to be translated by the system can be assumed to contain not more than a tolerable number of phrases and sentences that are bound to be left untranslated or (what is more serious) wrongly translated. I have no opinion on this question.

Neither of the two points I have just made is of course original. I am concerned merely to reaffirm their continued validity.

Let me now turn to the more specific questions raised by the present "feasibility" study. Much of the earlier work in the field of MT was based on a totally inadequate conception of grammar. Considerable advances in syntax have undoubtedly been made over the last decade. It is therefore reasonable to enquire whether the models of syntax that have been proposed recently provide any surer basis for MT than did the earlier, now obsolete, models. In one sense, the answer to this question must be positive. By this statement I do not intend to imply that there is now some possibility of an algorithmic determination of the syntactic structure of input sentences, although there appeared to be no such possibility before. I do not believe that the situation has changed very much in this respect. What I mean is simply that linguists designing computational procedures for syntactic analysis now have a better idea of the range of phenomena that they need to take account of than they had a decade or so ago. Much of the current work in syntax, however, would seem to be irrelevant to the problems confronting MT-workers. For example, it is difficult to see how a decision for or against the "lexicalist" position could have any implications for the design of an MT system. Nor is it obvious, in general, that a model of grammar that has been proposed by linguists without reference to the problems of translation and the practicalities of computation should be the best model for MT. We are, in any case, as far from agreement about the formalization of grammar as we ever were, and we may even be further.

One of the more striking developments in linguistics in the last few years has been the increased attention given to semantics and, more particularly, to the integration of semantics with syntax. Once again, it must be asserted, on any objective assessment of the current state of semantic theory, that such progress as has been made in this field seems to be almost totally irrelevant to the practical problems of MT. I have already alluded to the question of universal semantic "features" (and my own scepticism on this score). This is but one of the many points of controversy among semanticists at the present time. Others have to do with the degree to which the non-occurrence or abnormality of certain combinations of lexical items is a function of the meaning of those lexical items and the degree to which it depends upon the belief-systems of speakers of the language. In the case of both of these questions (and of a number of others that are currently being discussed), decisions we might come to independently of computational considerations seem to me to carry no implications at all for the design of a working MT system. It might well be that "real world" information relevant to the resolution of certain foreseeable ambiguities in the source language would be more conveniently coded in the lexicon in an MT system. If so, that is a sufficient justification of the procedure. Whether the semanticist would agree that "real world" information is rightly regarded as part of the meaning of lexical items on theoretical grounds is, in this context, of no consequence.

To summarize. If by FAHQT we mean a "fail-safe" system which accepts for translation texts varying widely in content and style, then there is no reason for us to be any more optimistic about the feasibility, or indeed possibility, of this than such critics as Bar-Hillel were some years ago. I would not exclude the possibility of constructing a practical system for translating scientific texts from one language to another to some lower, but perhaps tolerable, degree of accuracy. Whether the construction of such a system is feasible or not, I cannot say. But if it is feasible, my feeling is that it will neither contribute very directly to, nor depend very directly upon, advances in linguistic theory.

Philosophy of Language and the Feasibility of MT:
A Position Paper

by

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Philosophy of Language and the Feasibility of MT: A Position Paper

If this contribution to the project is to be considered a kind of crucial insight to be provided by the present author representing philosophy to the question of the feasibility of machine translation, it will, I fear, be a disappointment. The most important reason for this is, I think, the perhaps embarrassing, though by no means obvious, consideration that the concerns of philosophers with the problems of language intersect with the issues important to machine translation only tangentially. In stating this position, I indicated that this relative lack of relevance has not always been obvious. Subsequently in this essay, I shall be able to illustrate this in particular cases. For the present, I think it will suffice to indicate the illusion of greater relevance than I believe in fact exists comes about from the habit of philosophers generally to speak in terms of extreme generality and at least partly as a result, to avoid consideration of those little pragmatic factors which usually distinguish success from failure. A successful marriage tends to be for a philosopher, either an arrangement in which some aspects of the relationship are satisfactory, thus making successful virtually all of those that end in divorce, or to be one in which each partner plays his role in an ideal fashion, a view which rules out virtually all existing ones. Since the philosopher usually neglects to make clear the highly systematic character of his terms, one can easily be misled into assigning far greater practical consequences to his conclusions than they warrant.

For openers, one might point to peculiarities of the translation situation which would seem to make machine translation in principle impossible. If one views the purpose of translation to be the production of a text in the target language which will be of the same approximate length as any arbitrary text of the source language and will share with the source material all features of a semantic and pragmatic character, will inform with respect to the same features and suggest the same features to the same degree, there is every reason to doubt that machine translation is possible. No program, suggested or in project, or any theory of how one might be constructed, has even the remotest hope of accomplishing this. Before however one yields too much to despair because of this, it should be pointed out that this result holds equally well for human "normal" translation as well. The present author is quite familiar with both Dutch and English and has indeed translated material between these languages professionally. The requirement outlined above in this case would however require being able to convey in a few words such subtle cultural features in the Dutch as their peculiar love-dependence-fear relation to the sea, the effects of high population density and the oddities of periods of Frisian cultural dominance and inferiority, to mention but a few. Since these features can scarcely be explained adequately in a full sized book, it follows trivially that short passages dependent on them cannot be translated at all in the intended sense. And this in a pair of languages extremely closely related. The point of this is not of course that translations cannot be done at all, but this particular "strong" sense of translation represents an ideal extreme which can only at times be realized. Now of course for some cases even this extreme can be realized and indeed by computers. The existing program at the Linguistics Research Center when provided with a standard Dutch-English glossary (provided this contains standard mathematical terms) and even a relatively partial syntax would have no difficulty translating in this "strong" sense page 7 of volume IV of the ENSIE encyclopedia (this is a portion of an article on set theory).

The difficulty with much, perhaps most, talk concerning machine translation by philosophers (and not only philosophers) is that so much of it is devoted to the problem "in principle." Thus Herbert Dreyfuss, holding phenomenological views of human intelligence, argues from the intentional nature of human language to the impossibility of translation by machine. Here our examples are, I think, instructive. In the phenomenological sense "intentional" phenomena covers a large range of psychological data, including on one hand processes known to be strongly rule governed to processes whose nature we have difficulty in formulating even for purposes of identification. As a result the encyclopedia page cited above is as intentional as the most difficult culture-specific poem. Yet as I indicated above, the problem of satisfactory machine translation of the page in question may safely be regarded as solved. It follows then that the argument establishes the untranslatability by machine only of those kinds of linguistic material whose intentional character cannot be approximated by a machine, from which follows that

(1) Not all linguistic material can be translated by machine--a conclusion consideration of human translation causes us to be willing to agree to even without the argument, for reasons we have cited, and

(2) Those passages which cannot be machine processed cannot be translated by a machine.

On the other hand we have arguments based on a very selected corpus of sentences combined with a limited glossary which, by identifying the familiar with the universal create the impression that relatively simple grammatical analysis together with a simply structural dictionary will suffice for the task. The impression thereby created is that context dependence is limited to what is involved in transformation grammar and the resulting ambiguities resolvable by dictionary rules of agreement. This attitude is for instance implicit in the discussion by Jerrold Katz. And, indeed, so they are, in a great number of cases. What is ignored here is that this common means of reduction of ambiguity is but one of the linguistic devices for this purpose. Even worse, this view requires that that there be discrete senses of words leaving us in a quandry as to whether e.g. bachelor = unmarried male and bachelor = cheerful fellow constitute one, two, three or perhaps even more senses of bachelor. It does not require much ingenuity to argue for any of the possibilities.

Essentially what I have been arguing here is that what philosophers of language have had to say about the possibility of MT (directly or by implication) are largely or completely irrelevant to the question of the feasibility of MT in the sense in which this question is of importance to MT researchers and users. (A partial exception must here be made with respect to Bar-Hillel, primarily because he has generally been concerned with the pragmatic, rather than the "in principle" questions.)

Now if the argument I have been advancing is correct and we cannot expect solutions of the feasibility problem from the philosophers of language, it does not however follow that these will have no contribution whatever. In a certain sense, the contribution that I foresee does not differ in its most general description from that which one might expect of theoretical linguists, although due in part to differences of professional emphasis, there is reason to suspect that these will differ in exact content. More specifically:

1. Since there are parts of language for which the MT problem is relatively simple and definitely feasible, it is to devise general description, or models, of these situations and to do so in as general a manner as possible. Some of this process is clearly grammatical and lexical, but at least part of it appears to be the same as, or very similar to, what has been termed logical analysis. In both the linguistic and logical areas, it is likely at this point to appear to the unsophisticated to be simply a matter of description, but in both we have found that the facts, at least as far as they are known, can be described in many ways, some indeed more promising than others. This process consists not infrequently in the embedding of the linguistic phenomena in a larger whole and in many cases can be helpful in pointing out the possibilities of useful "normal forms" for the purpose of representing information within the computer.

2. Since, as we have seen, certain linguistic phenomena make excellent translation extremely difficult and in some cases _de facto_ impossible, whatever may or may not be the case "in principle," relative success in mechanical translation is very likely to depend on our ability to understand what these phenomena are. This is so, not only in allowing us better to estimate what it is simply uneconomic to try and dishonest to promise, but also because it may give us convenient guidance concerning useful modes of man-machine organization. Let me illustrate this. Most MT programs depend critically on a lexicon which in one way or another contains semantical and usage context notes. The normal way in which this information is used is to systematically utilize agreement information together with perhaps some additional information explicitly fed in--e.g.

this is an article on petrology: dialectal readings will be improbable. One kind of application which occurs with some frequency in language use (although certainly more common in verbal than written language) which is usually completely ignored is the use of earlier passages to help indicate the meaning of later ones. In normal use the mechanism is apparently not usually a direct effect linguistic context matter, but rather seems to work by modifying the expectations of the hearer/reader. This observation suggests on one hand the possibility that some improvement in ability to interpret and hence presumably to translate may be achieved by assigning and in some way modifying probabilities of one reading over another. Unfortunately at the present time, our knowledge of anything beyond the general outline of this interpretive process is so slight that it is difficult to even begin to answer the questions that must be posed before we can use it on a machine. For example, presumably if this technique is to be used we need to attach to our glossary initial probabilities of readings. How do we get them? Do we scan a large corpus? Ask specialists? Ask "men on the street"? Once we have them, how strongly should a confirmed "improbable" reading modify the probabilities? Finally, it appears pretty clear that any such modification should degrade to the normal probabilities (assuming we have them) with the passage of time. But how fast should this occur? In short considerations of the phenomenon and its apparent structure tell us in a rough way what we would need to do to implement this and also specifies a large number of difficult problems which would have to be overcome to do this. But there is at least one additional advantage that can be gotten out of this consideration. Suppose we wish to seriously consider having our translation procedure accommodate the feature we have been discussing but are pessimistic about our ability to easily overcome the difficulties indicated. This raises the question as to whether we might not be able to get part of the advantage with the help of human intervention. If we look into this problem, we can conceive of at least three possibilities. We might as a variant of pre-editing have an expert call out unusual readings in advance. We might have the machine call out alternatives not determined by agreement and dominance rules. Finally, we might have either a lay reader respond to the output indicating which readings appear wrong or an expert reader compare the input and the output. Assuming we wish to explore one of the latter two alternatives, say even the last one, it is clear if we are going to accommodate our phenomena that we will be going into a mode of man-machine cooperation in translation which has rarely been considered. That is, instead of construing the function of the operator as primarily analogous to that of a normal editor, he will, by virtue of the fact that his remarks (to have the effect indicated) must be fed into the computer's lexicon in some appropriate form and modify the computer's, presumably second-pass, output, become part of an iterative loop with the computer.

How practical this alternative may be I would not at the moment presume to guess. It does however illustrate one of the possibilities for further development. In passing it is worth noting that this (in common with several other ideas which I suspect will be worth examining in the next years) represents an abandonment of the picture of MT which has been more or less dominant: namely, the picture of a process in which human participation is limited to the design, but is excluded completely or present only at the fringes of the operation of the system. There is of course no reason to conclude from the obvious desirability all things being equal of such an arrangement to the conclusion that if such a system either cannot be designed or (more accurately) if designed is only of limited use, that the possibility of computer participation in translation ought to be abandoned completely, any more than the unfeasibility of completely eliminating human interaction on tactical data systems leads to the corresponding conclusion.

In concluding my discussion, it may perhaps be worth while for me to indicate those features of language processing concerning which I believe there to be some reason to hope for some contributions from general or philosophical considerations of language to MT--perhaps it may now or soon be advisible to signify the position I am taking, in this respect in agreement with a growing number of people, among others Bar-Hillel and Kay, by re-christening it as machine assisted translation.

1. I think that there is likely to be some advantages to be gained from systematic work on question answering models, both the logically oriented projects based upon the predicate calculus or combinatory logic and the more linguistically oriented ones.

2. The information incident to the performance of certain types of linguistic acts, most especially those of the type Austin terms illocutionary, frequently provides special uses in the interpretation of utterances. Since certain verbal forms are associated with these, in any event typically (it is much more difficult to argue for invariable connections here), one can see the potential utility of the translation program making systematic use of this. Some of the basic linguistic work incident to this is being pursued. If useful in MT, one would anticipate that it would be so primarily in creating greater flexibility in definition of the relevant context. There are indeed many problems, including the

strong likelihood that illocutionary acts are rather strongly culture dependent, so that it may be that then, for instance may perhaps be no equivalent illocutionary act in a society without feudal antecedents of the giving of ones "parole" or "Ehrenwort". Even this type of situation might however enable (assuming one could find appropriate ways of storing and referring to the information) improved disambiguation, such as perhaps avoiding translating the Dutch "Wees een heer in de verkeer" (a safety slogan meaning "be courteous in traffic") as "Be a man in traffic." In passing let me remark that the implicit identification of illocutionary acts with pure performatives (i.e. locutions which in the first person _per se_ perform the act to which they refer, such as "promise") which plays a strong role in Austin and some of his followers seems to me probably misguided. I note that Alston appears not to do this--and that an enumeration of illocutionary acts is in my view an outcome and no an input of a theory of illocutionary acts, certainly of one likely to be of use.

3. As I indicated above, the phenomenon of use of an expression primarily or exclusively to alter in the succeeding context appears to have some promise for MT (as I indicated above). It should perhaps be remarked that this appears to be closely related in many applications to the disambiguation processes connected with specifying the subject-matter.

4. Finally (for the present purpose), there is the phenomenon in which the hearer (or reader) expects under normal circumstances the speaker to be reasonably attempting to fulfill his apparent purpose--to inform about the matter at hand, to help solve the problem, to induce the behavior he wishes, or the like, as the case may be--and interprets his utterances accordingly and furthermore the speaker relies on the expectation that the hearer will do so. (In a somewhat oversimplified description, Bar-Hillel

has recently referred to this as the requirement of the "good will" of the hearer.) Among other things, this results in the strong tendency to reinterpret apparent tautologies like "men are men" as non-tautological. This is of course related to the phenomenon discussed under item 3. It should be remarked that this phenomenon is by no means limited to the assumption that the speaker will not gratuitously contradict himself--although it includes this--but also that he will on one hand agree with the information both he and the hearer accept as "obvious," but even that unless there is some doubt about it, that he will not state the obvious. Because this extremely great burden of knowledge about the world which this would appear to place on the computer, this phenomenon would appear to rule out MT irretrievably (and, in private conversation, Bar-Hillel has recently expressed himself along these lines to the present author). That the situation is not clearly quite this bleak appears to me to follow from the following considerations. Firstly, the material of primary interest for MT--in particular, scientific articles and the like--may not be quite as open-ended as all that; the example of the encyclopedia article shows in any event that there are times when it is not. Secondly, when the translation occurs between related cultures, the process may frequently have only secondary effects since the interpretive process in one language will then simply parallel that in the other, reinterpreting and disambiguating ususally in exactly the same way. Thirdly, this process and the related one referred to under item 3, can not infrequently function to "save" what would otherwise be a poor translation. For example, translating the English "I'm sorry" into Dutch as "het spijt me" (which indicates substantial regret and not mere simple apology) in circumstances of simple apology, usually does not occasion any misunderstanding (since the normal interpretation gets rejected as inappropriate). We accordingly get a non-colloquial translation which in terms of information conveyed remains adequate. Finally, the possibilities of human interaction to provide the required information on an ad hoc basis, either by intermediate discussion or by the kind of successive attempts editing discussed above may be of substantial help. Accordingly, the phenomenon ought to be rather the occasion for more intensive research.

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Toward a Theory of Computational Linguistics

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1. Two Kinds of Models

To begin with, I would like to assert that computational linguistics (henceforth: CL), despite its qualifying adjective, has to do with human behavior, and, in particular, with that subset of human behavioral patterns that we study in linguistics. In other words, the aim of CL as a science is to explain human behavior insofar as it avails itself of the possibilities inherent in man's faculty of speech. In this sense, CL and linguistics proper both pursue the same aim. However, there are differences, as we will see shortly; for the moment, let us just establish that CL can be considered as a subfield of linguistics, and leave the delineation of the boundaries for later.

An important notion in behavioral sciences is that of a model as a set of hypotheses and empirical assumptions leading to certain testable conclusions, called predictions (on this, cf., e.g. Braithwaite 1968; Saumjan 1966). I would like to call this kind of model the descriptive one. "Descriptive" here is not taken in the sense that Chomsky distinguishes descriptive adequacy from explanatory adequacy: indeed, the function of the descriptive model is to explain, as will become clear below. However, there is another respect in which the descriptive model reminds one of some of the characteristics attributed to Chomskyan models: it need not be (and should not be) considered a "faithful" reproduction of reality, in the sense that to each part of the model there corresponds, by some kind of isomorphic mapping, a particular chunk of "real" life. In other words, this descriptive kind of model does not attempt to imitate the behavior of its descriptum.

The other kind of model I propose to call the simulative one. As the name indicates, we are dealing with a conscious effort to picture, point by point, the activities that we want to describe. Of course, the simulative model, in order to be scientifically interesting, must attempt to explain; a machina loquax, to use Ceccato's expression (1967) is no good if there is a deus in machina. Although the idea of building homunculi, robots and what else they are called is not exactly a new one, the advent of the computer made it possible to conduct these experiments on a hitherto unknown scale, both with regard to dimensions and to exactitude. In fact, one of the popular views of the computer is exactly that: a man-like machine. Interestingly, the fears connected with this kind of image (such as an impending take-over by some super-computer like HAL in the movie "2001") have their counterpart in certain objections that are sometimes voiced against the other kind of model, the descriptive one: namely, that it de-humanizes human activities (such as speech), and establishes a new kind of man, made in the machine's image: machine-like man. Below, in section 3, I will discuss some of the implications of these views for computational linguistics; but first I want to raise the question: what importance do the two kinds of models have for linguistics itself?

2. Competence and Performance

The distinction between competence and performance in linguistics has been belabored often enough to let me squeak by here with a short restatement of Chomsky's remarks in Aspects (1965: 4 et pass.): competence is the speaker's knowledge of a language, performance is what he actually does with his knowledge in a given situation that involves linguistic activity. A theory of competence, Chomsky says, is not a model of the speaker-hearer; according to the distinction made in section 1, above, I would rather say that it is not a simulative model, but a descriptive one. In other words, the model that is a grammar does not attempt to explain linguistic activity on the part of the speaker or hearer by appealing to direct similarities between that activity and the rules of the grammar. Rather, the activity of the speaker (his performance) is explained by pointing to the fact that the rules give exactly the same result (if they are correct, that is) as does the performance of the speaker-hearer: the set of all possible utterances of a given language.

Although a theory of performance thus is closer to the idea of simulating an actual linguistic situation, it is by no means identical with the simulative model. Rather, simulating actual linguistic activity depends on such a theory for its success; without it, a simulative model will be of little interest to linguists. To take an example: in any concrete linguistic situation there will be a lot of "unexplained" phenomena, such as hemming and hawing, false starts, anacolouths, etc. I feel that Chomsky is wrong in ascribing all of this to what he calls performance: linguistic theory should not account for these aspects of speech (they belong more properly in what one might call "corrective linguistics"). A simulative model wanting to represent this kind of "performance" would be a waste of energy and time.

What, then, is the proper object of a theory of linguistic performance? To understand this question is to answer it: if performance by definition is actual human activity, then linguistic performance is activity exercised by humans in the form of speech acts. In terms of the restriction made in the preceding paragraph, our "ideal" performance is that activity minus irrelevant "noise". Notice that this ideal performance does not coincide with that of Chomsky's "ideal speaker—hearer" of the language: as I understand this person, he is some kind of linguistic Superman (with unlimited memory, boundless embedding facilities, etc.). In other words, Chomsky's "ideal speaker" reflects competence rather than performance (in Chomsky's sense). To take a very simple example: the set of sentences generated by a grammar is potentially infinite; this is a fact of competence. However, any actual speaker or set of speakers will always generate some finite subset of the set of all possible sentences: a fact of performance. On a more sophisticated level, consider such questions as: why is it the case that regressive embedding beyond a certain bound is unacceptable? Chomsky calls sentences such as The rat the cat the dog chased killed ate the malt "perfectly grammatical" (1963:286); true enough, if one understands by this term: generatable by a competence model. But a performance model would have to incorporate some restrictions by which these "improbable and confusing" sentences (Chomsky, ibid.) would be ruled out. Actually, much of the research in the fields of psycho-, socio-, neuro-, etc., linguistics deals with performance; it is my thesis that computational linguistics, too, is a province of the same realm.

3. Competence and Performance in CL

The next question to be answered is: how do these theoretical considerations reflect on past and current work in CL? Until recently, very little attention has been paid to the performance aspect of CL. The only really large-scale computer-aided research in performance has been concentrating on machine translation and related areas. The lack of success that characterized these efforts has been material in turning off research funds as well as researchers. The result has been that CL workers now mainly direct their attention to such questions as: how to implement grammars on the machine; and: how to let the machine take over some of the work that linguists traditionally have done by hand? An example of the first kind is the transformational grammar developed by Friedman c.s. at Ann Arbor, formerly Stanford (1968 et seqq.); work in the second category ranges all the way from fairly unsophisticated and theoretically uninteresting "book-keeping" and "fact-finding" aids to theoretically motivated work in the development of syntactic and phonological rule testers (e.g., Londe & Schoene 1968; Fraser 1969). Common to this type of research is its ancillary character: these models (descriptive) purport to be an aid in the establishing of a theory of competence. As to performance (and, by inclusion, simulation), it is interesting to note that some of the more worthwhile results of MT research fall in the area of competence, too. I am thinking here such by-products of MT as context-free and context-sensitive recognition procedures and their theoretical foundations (as explored, e.g. by Kuno, Greibach, Griffiths, Petrick, Peters and Ritchie, and, most recently, Woods (1970)). The results obtained in this area have certainly helped to clarify the theoretical issues involved, and as such, are of great value. But (as competence theory in general) they have not stimulated research or clarified any of the problems in the area of performance (except, of course, indirectly inasmuch as any theoretical development in one sector affects the whole field).

On the other hand, computerized efforts directed at simulating human linguistic performance cannot boast of any great achievements either. The fate of MT may have acted as a deterrent, but cannot be said to be the only reason why theoretical research has shunned, to a large degree, questions of simulation. In linguistics, in particular, the domineering trend of theoretical research was, until recently, to stay clear of what goes on in the speaker-hearer. As I pointed out above, in a sense it is perfectly true that a grammar is not a model of what is going on in the speaker's head; as Chomsky told the world in *Syntactic Structures*, "a grammar does not tell us how to synthesize a specific utterance; it does not tell us how to analyze a particular given utterance. In fact, these two tasks...are both outside the scope of grammars..." (1957:48). It should be kept in mind, though, that the grammars discussed here are concerned with competence, and that performance, in early generative grammar, was thought of as something less than ideal. I have the feeling, however, that the Manichaean streak which accompanied the distinction competence-performance at its birth is about to lose its power, and that competence now is seen as relevant only inasmuch as it can explain performance. But why talk about a theory of performance at all, then? Would it not be possible, with people such as Bar-Hillel (1970), to abolish the distinction altogether, and say: "competence is the theory of performance", or something similar? In the following, I will attempt to show that a theory of performance serves a purpose of its own, dependent on, but distinct from a theory of competence.

4. A Tale of Two Machines

In this section, I will conduct a Gedankenexperiment.*) Let us imagine two computers (or two computer programs), one (A) with the characteristics of a competence model (e.g., a system analogous to the transformational grammar described by Joyce Friedman), the other (B) resembling more or less Ceccato's machina loquax (see above, and also Mey 1968). Let us furthermore concentrate on the accepting part of the program, and try to figure out what happens in case the machines are confronted with a sentence that does not conform to their specifications. To take a concrete example, take the sentence: Colorless green ideas sleep furiously. Suppose A has built-in restrictions that, among other things, state that the subject of sleep has to be [+Animate], that the adjective green selects a [+Concrete] noun, and so on. Since the sentence presented to A violates almost all of the given selectional restrictions, the result would predictably be that A prints out a "reject" message, possibly with the reasons for rejection attached.

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*) The basic idea behind this experiment is due to Schank (1970). Schank makes his purpose clear as follows: "...the notions of acceptability and grammaticality are part of the justification and purpose of transformational grammar. Our purposes are entirely different. In terms of analysis we are concerned with assigning a conceptual realizate to a string." (1970:41)

What would our "Zwittermaschine" (Klee 1926) B do? Since B is a model of a human, and expressly purports to imitate human behavior, we can look towards a human hearer to obtain an answer. (Klee wouldn't lie). I think it was Arch Hill who first remarked that such deviant sentences sometimes are very well received by humans; in some of his experiments, students thought sentences like the above to be not only "modern poetry", but "good modern poetry" (Hill 1961). There is also a persistent rumor around that Dell Hymes, having read Syntactic Structures, promptly sat down and conceived a poem whose first line read: "Colorless green ideas sleep furiously, ...". Not to mention, of course, that all-time status symbol, the bumper-sticker carrying the same text and serving to fatten the pockets of some enterprising graduate student, while providing the more well-heeled members of the trade with a convenient shibboleth. To come back to our machine B: under the given presuppositions, it would have to find some way of imitating this human behavior, so disturbing to the creators of the selectional restrictions designed to produce the ultimate impossible sentence. For let us face it: there is no sentence so impossible that some human, in some devious way, cannot assign a possible interpretation to it. A quick glance at modern poetry will convince even the most incredulous (see also an article by Joseph Featherstone in *The New Republic*, 11 July 1970, "On Teaching Writing", where some interesting experiments in teaching children how to write poetry are described). This is not to say that selectional restrictions are for the birds (not even the one sitting perched on the leftmost handle of Klee's machine); only that it seems to be an innate human trait always to try to make the best of seemingly impossible linguistic input. If a *machina loquax* (or *audiens*, for that matter) wants to be true to its name, it will have to imitate this kind of behavior, and by doing so, explain some or all of it.[1]) And at this point I wish to discontinue the Gedankenexperiment, since I do not know how to make my machine do all this. But I hope to have made the issue clear: a simulative model, such as the one described, is different from a descriptive model. The difference becomes even clearer when one tries to implement both models on a computer. The simulative model requires a theoretical base of its own, since the theory of competence, by its own assumptions, rules out some phenomena that were described as typical for the human-like device. Conclusion: if CL wants to address itself to problems such as the ones involved in our little experiment, it will have to provide a wider theoretical base than the one accepted by most CL workers thus far. What we need is a theory of performance with special reference to CL.

5. Some further perspectives

In this final section, I will try to briefly indicate some of the areas in which I think a performance theory will be of use to CL. I will not propose any concrete solutions to any problems raised. The only aim I have set myself here is to provide some central perspective that I think may be fruitful to those working with the actual problems.

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[1] Of course it would take a machine both *loquax* and *audiens*. So why not *audax*?

As a general preamble, I would like to discuss the question: what do we want to use CL and CL methods for? If the answer is: as an ancillary to theoretical linguistics, i.e., as a practical aid in solving some of the problems that theoretical linguistics poses, then the theory of CL is simply the theory of linguistics. Applications of this theory include, on the one hand such uses as grammar testers, on the other, such purely mechanical aids as automated dictionaries, programs for finding certain morphemes in a corpus, etc. If, on the other hand, the answer is: to implement and perfect actually working models of human behavior in the area of speech production and recognition, then CL needs a theory of its own. Some of the aspects of such a theory are covered, or should be, in what one might call "general robotology" (for some ideas on this, cf. Simon (1968)): questions pertaining to the interaction between robot and man, or even the "computer use of human beings", to paraphrase Wiener. Another general question is that of the degree of fidelity in simulation of human behavior, and the best way to implement this simulation. For example, what exactly does it mean: "to achieve a point by point imitation of human behavior"? Surely we do not want to reproduce certain states of the human that we consider irrelevant to the simulated process? In actual speech production, to take one example, we may very frequently be confronted with poor performance on account of extraneous conditions (colds, objects in the mouth, drowsiness of the subject, etc.) For a linguist, there is little point in examining and wishing to simulate these conditions. True, in marginal instances abnormal conditions may throw light on certain otherwise obscured processes; but this is not usually so. But even abstracting from these cases, there are areas where the difference between a competence approach and a performance approach manifests itself in the simulative set-up. Take again the example of embedded sentences.

Despite the fact that the recursive embedding rule permits unlimited embedding, actual sentences will always be finite, hence contain a finite number of embedded clauses. Hence the question arises: can we set an upper bound for embeddings such that, for a particular sentence, the depth of embedding will not exceed that bound? And, more importantly, how can we linguistically motivate such a decision?

Certain problems in the field of information retrieval have affinity to certain linguistic performance problems. For example, given a certain input to a question-answering system, how can one minimize the number of spurious answers, especially in the case of an imperfectly formulated question? Parallel to this is the problem of perfect understanding of imperfect questions by humans: how much do we really need to identify a given question and produce the correct answer? Traditionally, computational linguists have proceeded from the assumption that one first had to decompose the structure of the sentence (the question), then assign it a semantic interpretation, which subsequently is matched with the data file and produces the correct output. However, it seems clear that humans, in their analysis of linguistic input, often bypass the syntactic part and go straight for the semantics. A very simple and inadequate illustration is found in newspaper titles; a better one is provided by the ease with which small children handle conceptual structures without having the syntax correct. My own under-fours often produce rather complicated "sentences" that are perfectly intelligible, although syntactically completely ill-formed (or non-formed). As an example, consider the following: far gå huse ikke (Norwegian), where the negation is placed at the end of the sentence: ikke ('daddy go house not', i.e., 'daddy don't go to your study'). The most interesting thing about my 3-year old daughter's negative sentences is that the negation particle invariably is placed at the end, no matter how long the sentence. Think of the savings in syntactic analysis time we would obtain if we had this kind of input to English question-answer systems! Furthermore, in a construction such as the one above, certain transformations (NEG-placement, e.g.) are clearly being omitted; but this does not affect the recognizability of the sentence by a human, or even by a computer that would be programmed to recognize deep, rather than surface, structures. Consider also the ease with which a computer could simulate such negative sentences, rather than spend costly time on rearranging the not's, nicht's, and so on that are the horror of freshman classes in ESL or German.

I am convinced that simulation experiments will prove to be extremely useful by pointing up phenomena about human speech use that at present are being obscured by the overly abstract approach to grammar of the last decade or so. Current research in applied linguistics as well as in the so-called "hyphenated" areas seems to confirm the trend that is apparent in theoretical linguistics proper: a greater concern for naturalness and directness in explaining the phenomena of language, with an emphasis on semantics rather than syntax, also in CL.

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Note added in proof:

Having completed the final redaction of this manuscript, I came across Christine Montgomery's contribution to the 1969 International Conference on Computational Linguistics (Sånga-Säby, Sweden), entitled: "Linguistics and Automated Language Processing" On p. 17 of her paper, the author advocates the necessity of a theory of performance along much the same lines as I do. Interesting is especially the fact that support for such a need is provided by some findings in the field of data retrieval rather than by linguistic considerations only. This is because the speaker-hearer of a language, in providing utterances, relates not only to his innate ability, but to "the total environment of the speech event as well". The result is that "speakers can and do process sentences which the grammar is not capable of generating; in other words, the relation between the sentences of competence and those of performance is not one of simple inclusion."

I regret having overlooked this important contribution, and herewith offer to Christine Montgomery my apologies, and to my readers, the advice to consult the paper in its entirety, as well as the references quoted there on p. 17.

MEANING REVISITED

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MEANING REVISITED

Eugene D. Pendergraft

The feasibility of building a mechanical system to perform high quality translation has been aired for some twenty years, not without emotion or result. At early conferences concerning the outlook of mechanical translation the discussants were apt to fly off the handle and go home in a pout, appearing to differ on the meanings of words such as "translation," "quality," "high" and "feasibile." But sponsorship supplied the lubricants that in time subdued their disagreements toward a comradery of benign misunderstanding. Volatile research groups formed. Some of them held together long enough to actually attempt preliminaries of the approaches they visualized, so that consequences of diverse methods and theoretical dispositions can be estimated.

I'm not a detached or objective judge of that grand experiment, which began with such irrational optimism that it had to end with irrational pessimism. I participated in it as passionately as the rest, since I'm convinced research of any kind requires passion.

As for objectivity, I came to believe that an objective point of view is out of place in translation research or in any inquiry whose subject matter depends on the very nature of meaning; that "objective" pertains to a particular class of meanings which, as a vehicle of scientific communication, denies access to the full universe of meanings.

These conclusions, once gleaned from my own research experience, profoundly changed my personal assessment of the prospects of high quality mechanical translation. What I want to communicate is the movement of my private judgments, neither detached nor objective, toward a point of view that you might think to be unscientific. At least, my viewpoint may seem at odds with that of empirical science and its derivative common sense.

I refer to the unique point of view of one sentient life, which each of us knows privately. Whether we like it or not, language often conveys private information and those conveyances, although supposedly at a minimum in technical documents, must also be translated. More important, only from a personal standpoint can we actually use language. This is our point of view as hearers or speakers of language who, in order to understand or to be understood, do make use of private information along with information which we as individuals infer to be accessible to others.

With discipline we can limit our conversations to public information, so carrying out a social contract that has worked well for physical sciences in their investigations of our common environment. But evidently the study of translation, except for superficialities, does not belong in that context. It involves, in addition, an investigation of ourselves.

Human translation is known to be most successful when the translators are informed about the subject matter they are translating as well as about the languages. This much is ascertainable by the empiricist's test: to translate well, one must understand. Why, then, should we look for "high quality" translation from machines that obviously do not understand; that merely manipulate rather than use language?

Indeed, one of the convincing findings of the past decade is the impotence of mechanical translation processes working on the surface of language and dealing almost exclusively with those linguistic features or forms ostensively available through empirical description. Processes of this kind have persistently demonstrated an inability to make useful selections among the immense number of possible constructions which give language its innate flexibility.

Defective translation processes do of course make choices; as Ida Rhodes gleefully pointed out, they produce "garbage." What we would find useful, instead, is mechanical selection closely analogous to patterns of human preference. Or, better still yet probably less attainable, we want a good and faithful servant: a machine whose choices would make ours more successful.

At the start, translation researchers did not express such lofty aims as these. They thought simple conditions for choices could be found in the text being translated and very near to the point of selection, in proximate words or phrases of the sentence momentarily under consideration. When such innocent expectations led to disappointment, investigators modified their attack in two different though complementary ways.

First, they set out to place more knowledge of the "source" and the "target" languages at the disposal of mechanical processes making choices. This was done initially by incorporating into the programming of the translation process some of what was then known about grammar. The result was a translation routine or "algorithm" specialized not only to particular languages but also to particular grammatical constructs. Because any change of grammatical detail required reprogramming of complicated relationships, translation algorithms were seldom completed in a programming sense; much less in a linguistic sense. Progress was therefore deadly slow.

Efforts to free researchers from the nuts and bolts of programming resulted in an understanding of the principles of "generalized" linguistic processes. That innovation made it possible to simply "store" grammatical data in the machine to be retrieved mechanically as needed as a basis for language choices. One result was a more rapid exploration of new formulations of grammar. At the same time, this new practice focused attention on theoretical problems besetting the mechanical translation enterprise.

The trick of generalized linguistic processes is that all of their choice-making is done in one of two ways. On the one hand, the choice is made with reference to some structural aspect of the deliberately constructed "metalanguage" being used by a given researcher to convey grammatical information about a specific language, say for example English. For the rest, the choice is based on the result of a comparison in which some grammatical detail so conveyed is matched against a possible instance of that detail in a specific discourse.

The former kind of choice is "formal" in the sense of being determined solely with reference to the metalanguage selected by that researcher for the purpose of describing grammatical details of English, or of another language.

In so far as one and the same metalanguage is used to record such details about a number of languages, all formal choices remain invariant among those languages. The latter kind of choice is "factual" by virtue of being specific to each language, and thus is variant among them.

However factual choices might vary from one language to another, the comparison process actually making factual choices can itself be generalized. That is to say, all of the auxiliary choices guiding the inner workings of each comparison can be referenced to forms or features of the metalanguage. From this, one can see that every choice necessary to specify the programming of a generalized linguistic process is formal.

It is precisely this characteristic of being separated or isolated from variant factual choices which justifies the label "generalized." This separation, now a familiar one in science, can be made as soon as the researcher has decided on the symbolic apparatus he will use for the purpose of recording the empiric facts imposed by his next experiment. For a linguist, his metalanguage is that apparatus. It makes explicit his theoretical bent from the point of view of scientific communication, since it embodies those invariant relationships which he hypothesizes for language in general, or for some family of languages whose factual details he intends to describe.

Although one can study the formal characteristics of individual metalanguages or of relations among metalanguages, attempts to "prove" that a given metalanguage is inadequate for the purpose of recording facts of language, or that one is better than another for this purpose, are in my opinion absolute nonsense. If they have meaning, such proofs are a part of the researcher's own decision process leading toward his choice of a specific metalanguage for the experiment he has in mind. His proof has nothing to do with whether he would have been right or wrong in choosing a different metalanguage than the one he finally selects. Most of all, his proof does not make his experiment unnecessary.

My personal assumption is that the selection of a specific metalanguage is not final, nor is the specification of generalized processes which that metalanguage allows. Both are theoretical choices in the design of an experiment about to be carried out. Both are preliminaries of that experiment, for which the metalanguage can and should be fixed and the generalized processes completed beforehand in a programming sense. Both, as instruments of formal as well as factual learning, can only be justified by the outcome of the experiment itself. For the present, the paramount goal of experimentation is to bring mechanical choicemaking into close conformity with human preferential behavior.

Clearly this goal has not been achieved, although much has been learned about language. Empirical studies of language, as such, have been expanded to also include the description of relationships among the things that language mentions, and among persons who are using language. All of this is symptomatic of a fuller awareness of the surface phenomena which human choices might take into account. So far, nonetheless, results of formal linguistic inquiry especially are tentative. Theoretical research has been unable to effectively demonstrate principles of an underlying formal organization which would shape factual findings about language in ways more useful for mechanical selection processes.

It can be shown, for example, that the entire translation process can be generalized through use of metalanguages capable of conveying interlingual relations of various kinds. However, this merely extends the idea of enlarging the machine's store of knowledge about language, an idea which by itself has not benefitted mechanical selection as much as researchers had originally hoped.

Accordingly, the second thrust of research on mechanical selection has been to widen the search for conditions attending choices. In addition to examining the expression undergoing translation, mechanical processes have been permitted to range over surrounding sentences, paragraphs, whole discourses, or data representing an increasingly extensive experience of language events located in the machine itself.

Due as much to disappointment as to expanding interests, mechanical translation research overflowed vaingloriously and became computational linguistics. This new domain of experimentation is a conglomerate of studies in which mechanical translation shares the limelight with information storage and retrieval, automatic extracting and abstracting, fact correlation, question asking and answering, and similar applications where language is manipulated mechanically. After an unsettling beginning, during which the old guard felt compelled to recant its former commitments, the new milieu of jargons did provide a sounder medium for testing language theories and methods than mechanical translation alone.

In consequence of this new opportunity to compare computational linguistic applications of various types, it has been noticed that mechanical selection comes closest to human patterns of choice in those instances where a little knowledge of language, things, or persons is brought to bear on an experience sufficiently extensive as a source of conditions for choices. In other words, mechanical selection appears to be improved by a better balance between mechanical analogues of experience and knowledge. Machines that ask or answer questions are examples of applications seemingly avoiding the narrow window of experience through which mechanical translation research tried unsuccessfully to squeeze great concentrations of knowledge.

In my opinion there are three lessons to be learned from this curious result of so much effort. The first concerns the way we might reasonably go about developing a mechanical translation system; the second concerns the type of system we might reasonably develop; the third concerns finding reasonable people to do the work. These problems are the ones requiring cogent solutions before feasibility estimates can be meaningful.

I think, however, that we have cause to doubt the optimistic assumption that men of good will must always reach similar conclusions on exposure to similar evidence, especially when part of the evidence is about themselves. By now it should be plain that no methodological consensus exists in mechanical translation research, without which comparisons of both formal and factual results are, at best, misleading. Before sitting down to make a second round of feasibility estimates, it might be proper to ask seriously why in our estimates thus far we seem to be getting "garbage" out of our own selection process.

One possibility is that, because mechanical translation researchers were gathered from a variety of technical specialties, we have not been looking in the same place for conditions on which to base our choices of method. By and large, it must be admitted that we have been a mixed lot, though sharing the prudent wish of every specialist: not to be caught on lame feet outside of his territory.

Another is the possibility that, as heirs of commonly accepted notions about the nature of man, we have been looking too much in the same place for the conditions determining our methodological choices. By preferring the narrow window of empirical science, we have avoided those taboo territories made uninhabitable by the "garbage" production of our predecessors.

As a prolific example of the latter I cite René Descartes, who ground his garbage so exceedingly fine to assay psychical as well as physical substances. Surely it is for lack of these psychic essences that machines are unable to use or to understand language; while we, brimming full, need only introspection to understand and master all of the configurations of our own choicemaking.

We have said a great deal in translation research about the dangers of anthropomorphising machines and so little about the dangers of anthropomorphising ourselves. What if it should turn out, as Charles Peirce claimed a full century ago, that we have no special vantage point to our own psyche, but must learn about that too by careful methods of inquiry?

Thus a third possibility is that our difficulties with mechanical selection are the result of self-ignorance, whose remedy should be a disciplined study of the ways we make choices ourselves. If in fact each of us is engaged in a quest for self-knowledge, then disparities in private understandings of the state of the art of human choicemaking might well account for some of the troublesome goings on in research which takes these understandings as its very ideal.

My personal conviction is that all of these factors are at work to make a second set of feasibility estimates as uncertain as the first. Before taking up the lessons which such estimates might turn to account, therefore, I consider it essential to make public some of the private assumptions unavoidably the source of my judgments.

PATTERNS OF HUMAN PREFERENCE

A summary like this one can do no more than lay out the bare bones of choicemaking activities which in each human organism are vastly convoluted and subtle. However, it is part of my purpose to spotlight the very hazard of abandoning oneself prematurely to mere facts so as to find solace in work of exceptional professional complexity. Formal investigation is, in its truest sense, an attempt to bypass variety in order to describe invariance.

This is not to say that formal inquiry, when it confines its interest to techniques of symbol manipulation, somehow escapes the same vice of specialization. Rather, I mean that my private fascination has been a train of formal thought with a different aspiration, running through the definitive studies of John Locke, David Hume, Charles Peirce, George Mead, John Dewey, Alfred Whitehead, Charles Morris, and others, and recently producing works of formal description like those of Jean Piaget, Lawrence Kohlberg, and Susan Langer.

The interesting characteristic of this line of formal reasoning is that it makes its theoretical choices increasingly on the basis of a disciplined interplay between formal hypotheses and empirical observations of invariants in human behavior. Thus it seeks to institute for forms an extension of those methods which successfully removed facts from the domain of fickle manipulation under control of human preference, and placed them under empirical control. In this aim it is the very antithesis of methodological opinion which sees theorizing as primarily a competitive arena for personal invention and argumentative directorship.

Piaget's study of the origins of intelligence in children is an elegant instance of this empirically disciplined formal method at work. It is consequently a good starting place for my summary, and a center line along which I will embroider my own thoughts or those of others caught up in the same intrigue of intellect.

From his observations of behavior in the human infant and child, Piaget isolates and describes six early stages of psychological adaptation. Each stage is evidenced by a characteristic scheme of choicemaking. It consists, on one hand, of the child's attempt to assimilate the environment by incorporating within his existing framework of knowledge and experience all new data given by his senses. On the other hand, it consists of his accommodation to the environment by using that modified framework as a basis for new acts. The existing adaptation at every stage, is an imperfect equilibrium constantly being repaired by successful assimilative and accommodative choices of its special kind, or being ruptured by unsuccessful choices of that kind.

Psychological adaptation, like the organic, can be explained in terms of relationships that are essentially ecological. Always and everywhere, adaptation is only accomplished when it results in a more or less stable organization of relations between an organism and an environment.

The point of supreme interest to us is the perspective from which Piaget chooses to construct his formal hypothesis. By observing stabilities in the child's relations to the environment as they appear from without, which is to say from the commonly accessible frame of reference of empirical science, the observer goes on to hypothesize how those somewhat unsettled ecological relations are felt from the personal standpoint of the child as his mind works out its first contacts with reality.

From the point of view of the investigator, then, factual data are those that can be observed to vary from child to child because they are imposed by environmental details that differ with the time, the place, the culture in which a person lives. Formal data by contrast are found to be invariant among children because, Piaget hypothesizes, these are necessary and irreducible data imposed on the child by his own genetically inherited biological organization. That is functionally the same for all of our species.

As a consequence one can deduce that, from the personal standpoint of a child, invariance is an aspect of experience distinguishing form from fact. And we have already seen that this same invariance is what the investigator might look for himself from the personal standpoint of his own research experience, when he is making theoretical choices.

Such a coincidence should warn us that formal hypotheses about the organization of human minds have direct methodological consequences which mark them as being basically different than factual hypotheses about the organization of the physical environment. When the investigation probes into the foundations of meaning and of understanding, there is a new need for consistency between any theory about the mind of the human subject under observation and that of the observer himself. What is hypothesized for the mental organization of the subject applies equally for the observer, and as a result can modify the choices of method open to the latter in his investigative role.

In short, the process of formal inquiry itself is seen to consist of a cycle of assimilation and accommodation. From observations of invariants in the subject's behavior, the observer assimilates new understandings of mental organization, to which he then accommodates his investigative behavior.

In this cycle of formal investigation, methodological choices can be recognized as instruments of formal accommodation for the investigator, just as theoretical choices are his instruments of formal assimilation. Choices of theory and method are both tentative and are "hypothetical" in the sense of self-consciously awaiting the test of use. Consequently, these are tools of formal learning for a mature intelligence, not for the infant just starting out in his feeble thrust toward consciousness of self.

The infant has his own instruments of formal assimilation and formal accommodation, for he can be observed to progressively modify the essentials of his scheme of choicemaking. Should that occur, one can tentatively assume that he has learned something, not about the environment, but about the organismic basis of himself.

Once the child understands the next stage of psychological adaptation, he prefers to use its new scheme of choicemaking, although it can be shown that he still knows how to use all of the schemes he acquired in earlier adaptations. The next stage is always a more desirable frame of knowledge and experience than the one before it, taking into account everything in the previous stage, but making new formal distinctions and organizing facts into a more comprehensive and equilibrated structure.

That each scheme of choicemaking is formally a prerequisite of its successor is argued by the observation that no stage in the progression of psychological adaptations is skipped. Each stage of adaptation has its own formal organization whose chief aspects my summary will try to illuminate. In addition, one should look for a progression of formal experience and knowledge in states of adaptation which are ever broader and more poised. It is this progression which allows us to think of the successive states as cumulative stages of mental development.

One must distinguish carefully between any existing state of psychological adaptation and the process of adaptation by which that state is changed. As Peirce was shrewd to notice, only when the investigator identifies formal inquiry with the process rather than the state, does it become necessary for his own state of mind to change should his investigative process succeed.

Formal reasoning has a dual purpose: to clarify the state of contemporary thought, and at the same time to benevolently undermine the world view that its fund of experience and knowledge represents. The aim of that benevolence is to carry forward the cultural process by including an established universe in a still broader and more stable one.

The central role of formal communication as a determinant of the state and the process of cultural adaptation has been explained by Mead and again eloquently by Whitehead. Each language has a formal component for talking about the everyday language to be used in talking about facts. Men also invent symbols for precise forays of factual description, as is well exemplified by the linguist's use of his metalanguage. Whatever the motivation, formal communication can either consolidate a cultural state by perfecting the symbols already being used to mention facts, or it can offer new symbols to further the cultural process by making possible the mention of facts until then unmentionable.

Whereas at this moment the need of the state of culture is to consummate an objective universe through the use of symbols that successfully organize vortices of objects in a continuum of time and space, the clear need of the cultural process is a new basis of symbolizing with which to organize a more comprehensive universe, incorporating subjective as well as objective facts, and a more equilibrated one by virtue of providing functional mechanisms for formal as well as factual adaptation.

How can a universe be symbolized to bring these neglected cultural ingredients to critical public purview? Langer has proposed that the basic symbols of such a world would name acts, and that the symbolic facility of a universe of acts would allow us to communicate about complex acts, composed of those elements.

The gist of the line of reasoning being pursued is that it is about the symbols of Langer's universe instead of those of Newton's universe which have become, after three centuries, so comfortable to a mechanistic sense of life. At first contact, a universe of acts is certainly a strange world; but then, any really new world must be strange. And a world view which aspires to incorporate the mechanics of formal adaptation has in added perplexity the responsibility to explain the circumstances of its own emergence. The job before us is to clarify the symbols of this unfamiliar world as best we can so that they can be used and tested against living and historic evidence, where strangeness has precedents.

Unavoidably, my summary will take up more mature stages of reflective thought following on the six initial stages of practical intelligence that Piaget looks for in the infancy and early childhood of individual men and women. It is in these markedly different settings that one can observe functionally analogous progressions of schemes of choicemaking. The invariant aspects of that progression might then be explained by an increase in human understanding of a biologically determined functional nucleus underlying and guiding consciousness.

Thus, the beginning of the process of psychological adaptation presupposes an existant biological organization, itself the product of an evolutionary sequence of genetic adaptation that incorporates hereditary factors having two quite different types of biological result. Factors of the first type determine the constitution of our nervous system and sensory organs, so that we perceive certain physical radiations, but not all of them, and matter of a certain size, and so on. Factors of the second type orient the successive states of psychological adaptation, and so have their result in the organization of a mind which attains its fullest and steadiest form at the very end of an intricate process of intellectual evolution, not at the start.

All of the various states and the process of psychological adaptation have in common the one formal aspect that, relative to an assimilated frame of experience and knowledge, the direction of every accommodation is such that it attempts to satisfy need.

Piaget maintains that needs and their satisfaction are mental manifestations of the complementary interplay of assimilation and accommodation as felt by any human being. Although from our personal standpoint need may seem primary, it is the internal organization of that underlying unity, the act itself, which motivates our day-to-day existence as well as our long term psychological development.

The theory of the act, making explicit the invariants to be found in every unit of human activity, would for a universe of acts set forth the cyclical relationships between assimilation and accommodation which are taken to be the functional nucleus of both factual and formal adaptation.

The act of Langer's world would not consist of movements in time and space as seen from some distant and impersonal viewpoint of a spectator, although such movements might indicate to the mind of a spectator the act of another mind. The symbol "act" would stand for any elemental or composite constituent of a whole but unique universe, one among others named by the symbol "mind," whose personal and partly intimate point of view would be felt as the very direction of the act.

More, the direction of the act would tend to satisfy the immediate needs of a state of adaptation by assimilating and accommodating to the organization of the environment. At the same time the direction of the act would satisfy the long term needs of a process of adaptation by assimilating the organization of the act itself toward an eventual accommodation which, through the mind's increased understanding of the principles of its own direction, would affect a new state.

This progress notwithstanding, Piaget contends as Peirce before him:

there does not exist, on any level of human consciousness, either direct experience of one's own mind or of the environment. Through the very fact that assimilation and accommodation are always on a par, neither the organization of an outer world nor that of an inner self is ever known independently. It is through a progressive construction, guided solely by the pragmatic circumstance that acts once committed to use either succeed or fail to be consummated, that concepts of the self within and of the environment without will be elaborated in the mind, each gaining meaning relative to the other.

The theoretical relationships between the several states and the process of psychological adaptation, as approached in the context of the theory of the act, is the core of the matter, therefore. It is from this connection that one may extract the multifarious method of inquiry indicated by Dewey and Bentley in their essay about knowing and the known in this new universe.

If the formal character of each successive state of adaptation is due to an increase in the mind's understanding of how the act is organized internally, then the invariants observed in each of those states should contribute new formal aspects for the theory of the act. Conversely, invariants observed in the act as such should help us to understand the theoretical relationships between the states and the process of adaptation.

Mead's analysis found the act to consist of three principal phases: the first a phase of "perception," the second of "manipulation," and the third of "consummation." But the method of analysis just suggested will find that every complex act has five functionally distinct phases which, allowing for an initial state, account for Piaget's basic progression of six adaptive stages.

This result would presuppose, for the theory of the relationships between the states and the process of adaptation, that it is an understanding of some new phase of the act which the adaptive process incorporates in the frame of formal experience and knowledge in order to pass from the existing state of adaptation to the next state of that basic progression. The efficacy of this view of the situation is given by various sorts of evidence.

Formal adaptation always appears as a growth of capacity in just one functionally distinct phase of the cycle of assimilation and accommodation implementing factual adaptation. This is at least consistent with the assumption that formal assimilation incorporates in the mind an understanding of the internal organization of that phase.

There is also an invariant order in the emergence of new phases of intellectual growth. The basic progression of six stages of adaptation exhibits that order in a number of quite dissimilar behavioral contexts, thereby assuring us that we are dealing with exactly five phases of functional capability, no more no less.

The initial progression consists of the stages of practical intelligence, where the five phases are first established as capabilities in the newborn child. Throughout the development of reflective thought that immediately follows, five functionally analogous phases emerge in the adolescent and young adult with the growth of representative thought. They occur again with the increasing capacity to verbalize subjective and objective facts contained in such thought, and finally with progress in formal verbalization. As the behavioral setting becomes more complex, the formal character of the phases is revealed with greater clarity. The cultural progression is accordingly the most elaborate setting from which one can extract the internal organization of each phase.

Within the internal organization of these five functionally distinct phases, one finds every capability needed to construct a viable theory of the act. That the phases are in fact constituents of the act is evidenced by the very possibility of that construction.

However, the sequence of phases defining the direction of the act does not turn out to be the same as the developmental sequence defining the order in which the phases enter consciousness. Evidently the first two phases of the act are understood one after the other, and then the fourth phase, the third, and the fifth. The process of adaptation always assimilates the phases of the act in this peculiar order to affect, through its respective accommodations to the successive mental increments, the basic progression of six adaptive stages observed in all of the behavioral contexts.

Even this unexpected state of affairs will be found to make sense in the context of cultural adaptation, where the developmental sequence can be recognized as a convenient arrangement for the transmission of social and cultural behavior across generations of individuals. To see this, one is required to consider the specific organizations of the several phases and the way they cooperate to determine the direction of the whole act.

To lay the grounds for that discussion, I must stress again that the most fundamental distinction for the new world we are exploring is not the one yielding a grid of space and time which makes possible the symbolization of movements in a physical environment. For a universe of acts, the basic distinction will be that made by Peirce of "potential acts" comprising the patterns of knowledge as opposed to "actual acts" being instances of those very same patterns which, in the relationships of their occurrence, comprise the experience of a given mind.

The dichotomy of experience and knowledge is a more comprehensive grid for our symbols than that of space and time. It makes possible the symbolization of acts making up a mind that is itself capable of symbolizing physical movements in an environment, as well as its own acts or acts of other minds.

Linguists will find this new grid familiar. It is the one by which known patterns of language, symbolized in their grammars, are ballanced against instances of those patterns which they symbolize in a given stream of speech. The dichotomy of knowledge and experience is nonetheless as wide as life. Every stream of existence contains sensory elements other than those of speech, which feed a ballancing act of magnificent dimensions.

The problem posed for the theory of the act is to explain the equilibrium that assimilative and accommodative processes maintain between actual acts of experience and potential acts of knowledge, given a stream of existence which is itself a sequence of actual sensory or motor acts, each instancing a successful or an unsuccessful consummation of some potential act among the elements of Langer's universe.

The resultant mind extends precisely as far as the equilibrium between experience and knowledge is maintained, whether the work of assimilation and accommodation is done by a single biological agent, or by a collection of them acting socially. The agent could as well be an electronic machine. This new world will be less skeptical of mechanical agents of mind than the present one, because it will look for mind in the equilibrium itself instead of in the agent.

Whatever the agent of a given mind, any flaw in its equilibrium will be "need." Any repair of disequilibrium will be "satisfaction." Persistent loss of equilibrium will be the nagging irritation of "doubt," according to Peirce the sole motivation for acts of inquiry which when successful attain not the truth of an external reality, but stability. For a universe of acts, therefore, any persistent stability in the equilibrium between experience and knowledge will be "belief."

In summary of the matrix of theoretical and methodological choices, I assume that the criteria of truth in a universe of acts are the immediate stability of its adaptive state and the long term stability of its adaptive process. These are pragmatic truths of fact and of form, respectively. The former relates ultimately to the organization of the environment; the latter, to the organization of the act. But there is no direct access either to a reality behind fact or behind form. Each is known or experienced relative to the other by means of complex acts which the mind itself constructs. The constituents of that construction are potential sensory or motor acts, the biologically or mechanically based elements of this unique universe, which is one mind among others. The sole source of the information guiding the construction is a given stream of existence, itself a sequence of actual sensory or motor acts instancing successful or unsuccessful consummations of those universal elements. And the organizing principles of the construction are those of the act, whose pragmatic method I will now discuss.

PRAGMATIC METHOD

Christopher Alexander, in his notes on the synthesis of form, cites a common engineering practice for making a metal face perfectly smooth and level. One inks the surface of a standard steel block, which is level within finer limits than those desired, and then one rubs the face to be leveled against the inked surface. If the face is not quite level, ink marks appear on it at those points which are higher than the rest. One grinds away those high spots, and fits the face to the inked surface again. The grinding and fitting are repeated over and over, until at some final fitting the entire surface of the metal face is marked by the ink, indicating that no high spots remain to be ground away.

The practice of fitting affords a useful way to think about the phases of the act. Because the act, too, consists of ongoing processes of assimilation and accommodation within which experience and knowledge are repeatedly shaped by putting their various parts to use, rubbing them against reality so to speak, in order to have them marked by success or failure as preparation for still another shaping.

It was Peirce who found out that the high spots of the mind are marked by the ink of success and the low ones by lack of it. Thus, fitting the mind to reality involves filling in the low points as well as grinding away the high ones. The mind had to be constructive in order to eliminate the holes and pitfalls of experience and knowledge. If men worked diligently enough at seeking out and building up the misfits, the entire stream of existence might become bright with success.

Although from this William James drew an elixir that pleased and encouraged a competitive society, the product he marketed under the label of "pragmatism" has since fared poorly in the popularity contest of ideas. That is significant for our inquiry, though not as an indication of some flaw in Peirce's insight. By the hard-eyed predictions that the actual practice of pragmatic method made possible, the course of its own acceptance has in fact been remarkably well borne out.

Stubbornly fixing its attention on the surprise of failure, pragmatic method was sure to be unpopular to every conservative trend of mind. That opposite practice, finding all of its reasons in the preservation rather than the creation of information, deliberately tries to avoid surprises and to explain away its own failures. For a conservative mind the sources of gratifying or noxious information are invariably felt to be outside of itself. In simple consequence, every form of conservatism directs its main purposes to preventing contamination of the specific place from which it sucks nourishment. To such a mind the purposes and attitudes of pragmatism have been and will continue to be irrational.

The conflict of rationality we are about to consider is the most exasperating one known to man because it stems from the direct opposition of creative and conservative assumptions about what information is, where it comes from, and how it is used. By comparison, all earlier crises of the cultural progression will have been mere squabbles among conservative minds in solemn disagreement over good and bad teats.

In the fifth universe, "information" is something to be transmitted across its space-time grid. The ultimate source of information is a material reality common to and encompassing all of mankind. The firsthand passage of information, by which it arrives in a brain that is essentially a passive receiver pretuned genetically to certain vibrations beyond itself, is called "observation." The brain stores up some of the information it receives and can also retransmit informative copies from its store by means of a conveyance of symbols that lodge themselves in other brains. This secondhand passage of information from one brain to another is "communication" or, for the young in passive receipt of a largess from the information store of society, it is "education."

The method of "descriptive" science, although less conservative than its predecessor, still locates the information source externally. Its works of observation are best done by a disciplined spectator who separates himself as rigorously as possible from all temptations of human purpose. The social status of the scientist, so engaged in carrying out his contract of detachment, is not unlike that of the priest whose nearness to God in the preceding social order called for all sorts of precautionary measures to insure the fidelity of firsthand information.

In general, one can identify information specialists at each stage of culture for whom contemporary men reserve their greatest veneration and suspicion. This highest peak of cathexis may now be explained theoretically by the need of every society to cluster around its fount of firsthand information in order to carry out the social act.

The necessary consequence of any change in the information source will be social reorganization, a period of turmoil during which new information specialists learn their roles, and users of their information scurry to the unaccustomed precincts of yet another defective metamorphosis. An improved equilibrium might then be felt by its participants as the preferred "order". Without that shared judgment, the new metamorphosis would fail. Society would revert to its former state, or would backslide down the cultural sequence to a regressive state within the scope of its remaining capability.

The pivot point of the adaptive process would appear to come when a society, or by the same principle a personality, feels the need to modify its source of information. This is the invariant to be looked for from the standpoint of the mind itself, even though our theoretical explanation holds that such a fundamental change is caused by an understanding of some new phase of the act being incorporated in the mind's functioning to thereby affect new ecological relationships.

Besides that our line of formal reasoning predicts that any new state resulting from an advance of the adaptive process will at first involve a reorganization of known facts. Thus the repair of intellectual progress is always felt by the personality or the society as a consolidation of mental holdings, in a word, as an "insight". Only after the introduction of a more comprehensive organizing principle can new facts be added to a reconstituted structure that has become at once broad and stable enough to receive them.

These conclusions are, in themselves, organizing principles of a personal world view emphasizing learning rather than doing.

Giving its highest priority to doing, the fifth universe uses its symbols to persuade other individuals or other societies what ought to be done. Inquiry is a garnering of information under stringent regimens that protect the quality of a product being pigeonholed away for unspecified future use in an advocative scheme of choicemaking. There, hard-fought positions are reluctantly abandoned under the shear weight of damaging evidence.

By comparison, the pragmatic scheme of choicemaking is one in which a real preference for surprises actually courts failure as a gratifying means to the shaping of an affluence of hypothetical creations almost lightheartedly sent forth in the hope that new truths might be caught in their net. The preferred symbols of the sixth psychological state belong in a context giving its highest priority to learning, and so they pertain to changing one's own individual mind or the mind of one's own society, not another's.

"Information," in the sixth universe, is something to be created against the grid of experience and knowledge by the agency of an ongoing organic process for which each mind's fragile stream of existence provides the indispensible clues. Those surprising instances when a given mind fails to achieve an expected objective are, in a world motivated by the need to repair itself, the necessary benchmarks for firsthand information being self-consciously designed to circumvent know misfits that are obstructing human satisfaction.

Hence the characteristic forms of pragmatic "communication" are to broadcast throughout the community all known points of distress and any helpful new designs by which past failures might in the future be overcome. One can readily see how such an innovative mode of communication will be disquieting when taken out of its proper context by a conservative state of mind bent on maintaining credible displays of tradition, authority or power.

Formal incompatibilities of conservative and creative views of information do indeed cause a "communication gap" with which the pragmatist, for his own part, is unable to cope. Advocative arguments will be perceived by him as "irrelevant" for two clear reasons.

First, a mind will not be persuaded by appeals to tradition, authority or competitive advantage once it believes that all "truth" is established by demonstrations of successful use. A persuasive rhetoric will be received disrespectfully as artless in the production of "false" designs, either untestable or long since disproven to a more receptive conduct of life. Seeming corrupted because of its higher resistance to corruption, the pragmatic mind will reject argument just as an argumentative mind had earlier rejected preachment.

Second, and more noteworthy of the pragmatic view of information, is its implication that an essentially conservative mind can be induced to learn by denying it the opportunity to overlook its failures of awareness. Mules of the sixth universe, once brought to water, will be taught to drink. The pragmatist's exorbitant desire to teach, as well as to learn, has not received the scholarly attention it deserves.

His discernment of this fresh possibility for improving all manifestations of the art of symbolic communication will inevitably call for new styles of speech and writing in which language is deliberately designed to stimulate a mind's own constructions through enrichment of its awarenesses. The more exquisite these new forms of expression, the more they will appear a mouthing of absurdities to minds steadfast in the obsolete notion that words transmit messages. Thus the result that Dewey prophesied: symbolic communication conceived as teaching will rely less on language and more on active displays that dramatize the student's own weaknesses and his own overlooked possibilities.

The goals of "education" will be attained by a variety of activities and situations especially designed to progressively awaken a mind at first so feeble that it would shyly act to protect its meager hoard of dependable creations, thinking them the gift of one or another fountain of charity. The stimulation of formal learning, while tenderly administered to the young and mentally impaired, will reprimand the laggard so remissive in his own mental betterment that he extends a nacent conservatism into adulthood.

Society in the sixth universe will not achieve the elusive goal of classlessness. It will prefer an order of psychological classes wherein a forefront of information specialists gather loosely around the sage to be students and teachers of one another. As for the sage, he will probably turn out to be a mathematician.

In Peirce's guess at the riddle of life, man's framework of experience and knowledge has been gradually broadened to include the "law" of the human act in its complementary relationships to the "presentness" of the environment. Mediating these two extremities of consciousness is "struggle," a conscious sense of learning in a collective mind appraised finally of its own creative act of inquiry. About that wellspring of information he says:

> ...there is manifestly not one drop of principle in the whole vast reservoir of established scientific theory that has sprung from any other source than the power of the human mind to originate ideas that are true. But this power, for all it has accomplished, is so feeble that as ideas flow from their springs in the soul, the truths are almost drowned in a flood of false notions; and that which experience does is gradual, and by a sort of fractionation, to precipitate and filter off the false ideas, eliminating them and letting the truth pour on in its mighty current.

Pragmatic method is more casual about forgetting because it has taken the act of creation in its own hands. The information specialist of the sixth universe will be a participant, immersing himself in a struggle to stabilize personal and social relationships which, in the pragmatic scheme of choicemaking, will give first priority to the satisfaction of human need. More tolerant but less egalitarian than his predecessor, he will characteristically create his own responsibilities without waiting for or wanting a contract or mandate.

One should not overlook the precocious act of genesis in Peirce's own hypothesis that a given mind can cross the formal chasm to this final metamorphosis only by incorporating to itself an understanding of the very process of forming an hypothesis.

From his singular assumption that forming an hypothesis is the mind's sole source of novelty, the whole fabric of pragmatic purposes and attitudes can be deduced. So I have deduced some of the formal character of pragmatic thought, hoping by my example to make you aware that formal accommodation is the result of the mind's own effort to work out the consequences of a revised conception of the origins of information.

This pragmatic line of reasoning will conclude that the adaptive process need not depend at all on a persuasive rhetoric to change the currently existing cultural state, nor in consequence to reorder the entire edifice of society as we know it. Pragmatists will act on that belief quite regardless of the fact that our society, like the one it replaced, will prefer to explain and work for social change on its own terms.

A failure to even consider the existence of an alternative theory of information, despite the damaging evidence of history, may be the Achilles heel of men engrossed in describing a common universe while jockeying argumentatively, and too often belligerently, for positions of material advantage in it. Their whole world might decay, not because they were greedy for dominance, but just because someone else had a sounder hold on information than they.

Be that as it may, it was his own pragmatic method and not the method of descriptive science which Peirce claimed with quiet immodesty to be the only way of settling belief that does not lead to eventual disappointment.

This prideful prediction is the weakest of his speculations. For paradoxically, through the very insight made possible by its sixth genesis, a mind would discover all of its previous metamorphoses to have been its own imperfect creations also. Such expertise in the art of information would itself be the imperfection of a Superman who, Frederich Nietzsche most accurately foresaw, was destined to walk among men as among animals and be ashamed.

I know how difficult it is to place much stock in these unwelcome speculations. You and I don't take philosophy seriously, being practical men. And that is precisely what pragmatic reasoning has to say about us. A penchant for getting "the job" done, a ponderous insistence on "the facts" when disinclined to make a decision, a heartfelt disdain for formal reasoning when it "gets personal," all are indexed in the fifth stage of the cultural progression among the characteristics of that state of mind most recognizable as ourselves.

I will try to convince you anyway that some baggy-kneed philosophers may have uncovered a real, honest to God, new universe in which a few minds are already living and you might live yourself.

Just do not ask me to describe this new universe to you, as though you were about to sit skeptically in judgment of my facts. I will merely decide that you are an inveterate spectator and hence too passive or too mesmerized in the art of objective reasoning for the pragmatic journey I have in mind. In any case, I have carried you as far as I can.

From here on it will be necessary for us to run together in the hope of breaking through to the sixth state of mind in which the phases of the act make sense. Should we succeed in that joint endeavor, it will then be possible to look back and see that among the reasons for proceeding in this manner there will be a revised estimate of what language can do.

You have your personal standpoint and I have mine. For the time being, suppose that our interpersonal relationship is the one explained by

Ogden and Richards, in which the symbols I produce can cause you to make acts of reference.

For Ogden and Richards the meaning of "meaning" involves a twofold orientation. My choice of symbols will be caused partly by the specific reference I intend for you to make and partly by personal or social factors. Examples of such factors are the purpose for which I am inciting you to make the reference, the proposed effect that recognizing my symbols is to have on your own purpose or attitude, and my own attitude as my symbols are being produced.

When you recognize my symbols, similarly, they may cause you to perform an act of reference and coincidentally to assume a purpose or an attitude which will be, under the whim of circumstance, more or less the one I intended. That hope for result is by no means certain.

Two chances for error are implicit in the double orientation itself. You may mistake the "content" of my reference, attributing to it some different subject matter than I intended. Or, you may mistake the personal or social factors which make up its "context." Either eventuality would distort the meaning I was aiming at, or you might fail to catch it entirely.

I do not mean to imply that, should an accident of communication occur between us, I would have conveyed more information to you than you were able to receive. Your sheer assumption that it was my purpose to transmit information to you would belong in the context of objective inferences. A current preference for that context seems also to have established the firm attitude that the only things men can converse about with any precision at all are the ones they can lay their hands on.

In this vein, for example, Ogden and Richards propose to lay their hands on symbols and their "referents" so as to converse propitiously about a relation, "truth," imputed by thought, but which thought alone could somehow not sustain. With telltale zeal to locate all worthwhile instruction in solid matter apart from mind, these investigators too, despite the many worthwhile things they say about problems of meaning, would not go so far as to explain the truthfulness of symbols in terms of mental organization.

The root of the matter is that every acceptable means of scientific investigation has been unable to locate minds, and hence thoughts, on the continuum of time and space. Popular belief tends to favor the inside of the head rather than the stomach, which had its day when men were hungrier. Until the right spot is discovered and demonstrated, it will be quite meaningless to speak of something "apart from" or "beyond" or anywhere positioned relative to a mind. Minds, as a result, have become disreputable in an objective scheme of things where they just hang around lousing up an otherwise impressive cosmos.

Pragmatists have been nicer to minds than scientists. For that kindness, I have labored to make you aware, there may be a troublesome price to pay. Protective mentalities of every stripe may have to get used to contrite pragmatic traits of character which evolve, without outside help, within a mind become so stalwart that it needs no cosmic plan beyond its own anatomy.

Within that frame of pragmatic reasoning, launched by inferences which work out the consequences of decidedly different cosmological assumptions than those customarily assumed by you or me, the established order has been found guilty of grievous errors in its thoughts and words. Were this not enough abuse for a society priding itself on being "scientific," pragmatic response to the second-rate status of minds in a material universe has, with its usual candor, judged the method of descriptive science to be itself an aberration of a conservative mind in need of shaping.

The specific complaint points to a society recklessly applying objective inferences to the content of human purpose. It cites a scientism which perpetuates the excess of enthusiasm of men in the Middle Ages who, having organized their entire cosmos in conformity with inferences about an omnipotent will, imposed purpose on everything that moved.

This criticism, as such, is a formal communication designed to broadcast a misfit in the way language is being used to express facts. On the constructive side it suggests that, to overcome incompetence in designing symbols, one should observe a scrupulous match between a reference and its context.

"Meaning" is the explicit match, utilitarian by nature, for which the specific inferences defining the context shall have been tested by successful application to the specific content referenced. From this harsh point of view, it follows, the only proven domain of objective inferences is in point of fact those contents of experience in which a person lays his hands, or his eyes, or some mechanical extension of them, on something permitting experiment.

To accept this diagnosis and cure, an entirely new world view will be necessary. Your cosmos and mine will have to be quite literally the framework of just one personal mind that feels itself to be participating, as a mere agent of the social act, toward the shaping of one or more collective minds.

Pragmatic criticism is not only addressed to a specific anatomy of information constituting a mind, it presupposes above all else that the signals of misshapen experience or knowledge are given systemically within that mind's internal organization. "Error" makes no appeal whatsoever to any other reality than a particular texture of one's own experience or knowledge just as such. Pragmatic explanation, similarly, is always posed in terms that ultimately recommend mental constructions.

Suppose, as an example of the latter, that acts of reference are taken to be the constituents of that immediate "perceptual" experience which in a given mind is felt as an orientation to what is now present ostensively, right at hand. Also imagine that, on this relatively secure foundation, a speculative extension is then built by the agency of acts of inference from the particular contexts matched to those specific contents being presented perceptually. Constituents of the resultant construction are "concepts;" the elaboration itself is the newest part of that mind's "conceptual" experience, felt as an orientation to things not present yet having import for some activity either being contemplated or in progress.

Giving this theoretical explanation its due would adduce, from the very mind committed to it, the consequence that errors of reference or of inference will, in general, beget malformed experience. Were such a faulty framework used to guide further action, the enterprise would culminate in acts prone to failure. Hence, in consequence of accommodative inferences tending to reorganize its own methods along pragmatic lines, the mind would finally conclude that, from its own personal standpoint, its own failures are its only signs of mistaken perceptions or conceptions.

To that personal world my symbols can carry nothing along with them except the skillful ingenuity with which I designed them and then launched them by mouth or hand, all the while guessing at your skill for using them to create information. Indeed, your ingenuity might be greater as a creative recognizer of symbols than mine as a producer of them.

As for the truthfulness of my symbols, I believe you will discover "truth" in them to whatever degree they stimulate and assist your own creative efforts. If they cause you to fail or carry you away from insight, farther than you would have gone by yourself, you will certainly judge them "false."

My symbolic designs can bring you no evidence, nor can they offer you proofs. They can only recommend how you might look for evidence in order to convince yourself that this revision of your present state of mind might improve the satisfaction of your everyday needs.

Then will you eagerly extract every scrap of evidence from which the further construction of your own experience or knowledge might profit. In your unique universe you will have to do all of the remodeling for and by yourself, and you alone will judge the result.

For my part, despite this ample domain of personal application, I see no reason why these same pragmatic practices will not also satisfy the needs of a society giving its highest priority to learning rather than merely doing.

Regarding the question of precision in the social use of symbols, I think you will agree that these pragmatic methods tend quite naturally to the happy hunting ground of mathematical reasoning, where especially critical minds can live out their cloistered days as students and teachers of one another toward the sole purpose of shaping up the formal component of explicitly constructed "languages."

Not only do the ministrations of mathematicians succeed admirably, they belie the empiricist's expectation that fact is a more stable foundation for society than form. The myth of empirical description to the contrary, science rode to its present glory on the back of mathematics.

Of course, mathematicians argue more than they would like. And if some of the symbolic designs they produce are named "proofs," I do not object. No mathematician has ever been known to accept one of those proofs from his cohorts without performing every reference its symbols specify, while passing judgment on each meticulous step for and by himself.

I therefore can hardly resist classifying mathematicians as the most excellent pragmatists of all. The things of importance are that their inbred practices have originated unusually reliable inferences in special minds which, since the time when mathematicians themselves could still believe they were describing abstract relations of rather etherial realms, have carried forward an explosive growth and variety of new constructive possibilities. Possessing a plentitude of alternative contexts, scientists have gone gingerly about the complementary task of fitting them to factual contents.

Thus the real route to exactitude in the scientific use of symbolism may have been presaged in the pragmatic view that, because facts vary, errors are at a minimum when men communicate about what is invariant among, hence within, themselves. As scientific hypotheses took on an increasingly mathematical character, the outworn trappings of a descriptive method were bit by bit discarded.

Like the earth-centered cosmos of the Middle Ages, was it necessary for our own world to be inverted before further progress was possible? To take the Peircian hypothesis seriously, you will have to identify it as having originated new organizing principles for known facts. It will have done the same sort of intellectual work as the Copernican hypothesis, whose emergence in the preceding state of culture portended the disintegration of an autocratic society and its reformation as a social order more favorable to industry.

That earlier transition, just as the one presumed to be now grinding to its sixth destination, was upsetting to the status quo. The spirits, spooks and fairies that moved the fourth universe were not cast aside easily.

Nicholas d'Oresme had observed that God had constructed the material world like a watch, which could then be left on its own, in no need of spiritual forces to move its various parts. A new theory of motion gradually emerged, helped along by Burdian, Albert of Saxony, and the philosophers of Merton College, Oxford. A direct precursor of Galilean dynamics, it held that any body when pushed is given an impetus that remains with it, or only slowly diminishes, if no obstacle is placed in its path.

But the law of gravity was never perfectly formulated by Galileo, who retained a conservative preference for circular movement. Even Newton had to resort to God to explain the maintenance of motion in the new universe then under construction.

It is astonishing how few new facts Galileo used in his argumentation to support the Copernican system. The experience on which he based himself was at bottom the same everyday experience that the Aristotelians had interpreted. He arranged this experience in a completely new way and made it more comprehensible. One would be mistaken to think that the "scientific revolution" of the Renaissance consisted in a triumph of the description of new facts over formal speculation. Not until the writings of Francis Bacon became the bible of the Royal Society did the collection of facts become hectic.

It was Descartes who finally banished active spirits from non-living matter once and for all to launch the world we know. The essence of matter consists in extension; the essence of spiritual principles, like that of the soul, in self-consciousness. According to Descartes, therefore, God does not operate in the cosmic process either. The behavior of the material universe must be explained on its own terms.

By far the most interesting consequence of Descarte's theory was its contribution to the disappearance of witch hunts, which had grown to incredible dimensions in both Protestant and Catholic countries. That is one sound indication of an unsettled world retrieved toward salutory equilibrium when intellectual preference turned away from asking "What is God's will?" and took up the question "How is the world constituted?"

Although the information source of Western culture had changed, the brotherhood of man under the fatherhood of God was not forgotten. An egalitarian order was essential to the advocative scheme of choicemaking that replaced the authoritarian one. As Dewey and Bentley remind us, the spiritual entities which had once inhabited dead matter took flight to new homes, mostly in the human body, and particularly in the human brain. They had been swept out of the way of progress, right into another sanctuary of mischief.

The new social order formed decisively around two information specialists, polarized in the separate responsibilities for objective and subjective choices implied in Descartes' dichotomy. The emerging mechanistic world view became the watchword of the eighteenth and nineteenth centuries, providing the theoretical basis for both poles of the new advocative scheme. Science, but also economics and politics, were conceptualized in terms of forces held in abeyance by counterforces balanced against them. Ultimately the forces of nature were balanced against the burgeoning will of man.

An oddity of the transition now attending the dissolution of a mechanistic world view built on polarity is that it will require a double genesis. Moreover, it was to be expected that scientists and educators, having been commissioned to a quiet concern for learning in an industrial society, would be susceptible to pragmatic influences in greater degree than active managers and makers of public and private weal.

A decline was foretold when George Berkeley, motivated by the fear that Newton's principles of absolute space, absolute time, matter and gravitation would threaten religion, doubted whether the words in which these principles were expressed even made sense. According to him, the only words that are meaningful are words that designate sensations. If the goal of science is to coordinate sensory perceptions, then it can make use of spatial relations only to the extent that these are merely relations between sensible bodies, and nothing more.

Out of the matrix of Berkeley's arguments came two fertile seeds. One is the distinction between the formal and factual components of language, now grown to the rank of a major preoccupation among philosophers. The other is the very method of approach by which scientific procedure took on its exclusively descriptive character, that of ascertaining, and only then interpreting, the data of sensation.

A new direction came pointedly to the surface when Immanuel Kant asked "What can we know about the world?" He had shifted the source of information toward a personal perspective despite his own conviction of the correctness of the Newtonian world view, which his own cosmological hypothesis had broadened. He sought to substantiate it. In his opinion the principles of natural science, such as those of causality and the law of conservation of energy, are unconditionally true because the mind thinks in Newtonian terms. This explains why we are able to comprehend nature; its general features are indeed our own work.

The spectacular growth of science in this century has armed a mechanistic world view with an abundance of arguments. Meanwhile, the forefront of scientific method has moved out on the much more mathematical course blazed by men like Faraday and Maxwell. Near the turning point Ernst Mach asserted the radical idea that science can, and should, do nothing but order experience.

Mechanism was struck a severe blow by Einstein's special theory of relativity, which introduced objective probabilities uninterpretable in terms of sensation. Later a coup de grace was delivered by his general theory, originating a curved space-time based on Riemann's geometry.

It should be noticed that my pragmatic reconstructions of recent history make no appeal whatsoever to an advocacy on the part of Peirce. Were he alive, I know he would resent my shabby treatment of the brainchildren he dressed so carefully. What matters after all is that, though sorely lacking the Madison Avenue touch, he did advertise these new constructive possibilities. That is all his pragmatic theory of social change required.

The universe of your own personal mind is one you know well. However, you have not thought of your social universe as being organized along principles of mental anatomy, and will doubtlessly think that further suggestion is silly.

Believe me, I share your annoyance. I have lived as comfortably on the grid of time and space as men did formerly in the lap of God. But a surprising thing happened to me one day on the way to the laboratory. There were people in the streets yelling about the misfits of our society, and it suddenly occurred to me I was being set upon by a bunch of pragmatists.

Now I am pretty sure that none of these ragamuffins had ever read Peirce. Yet they made it crystal clear that they were intent on shaping the collective mind of their OWN society, and also their OWN individual minds. Their inquisitive attitudes gave much of Nietzsche's holy Yea to life seekings its OWN will to win its OWN personal world, though it be the world of an outcast.

Yes, they did seem painfully aware of Nietzsche's metamorphoses by which the human spirit becomes a camel, the camel a lion, and the lion at last a child. Along with their childlike craving for novelty, they were impatient with bearing burdens or doing combat.

I call your awareness to these untidy deportments because of their unheralded intrusion upon a world so desperately intent on persuading OTHER societies, and hence OTHER individuals, what ought to be done.

I submit that attempts to explain these surprising events in the context of the existing cultural state have so far not been very satisfactory. By way of contrast, the same happenings fit so well in Peirce's hypothetical universe that I have gradually been compelled to regard its peculiar galaxy of pragmatic purposes and attitudes as less a philosophical abstraction and more a veritable contagion of contemporary men and women.

I came as a pedant to the pragmatist's world, but have since met some of its native inhabitants for whom its exotic ways of behaving appear as natural as breathing. At any rate, I am constantly being amazed as my hard-won conclusions are judged by these people to be just ordinary common sense. The really interesting question, therefore, is where these purposes and attitudes came from.

I, for one, am not very impressed with the explanation that the Devil has been busy corrupting affluent children who were abandoned by permissive parents to permissive school administrators. I do admit that supernatural corruptors might be having a fretful problem now that the Communists have grabbed so much of their work. But since the Communists themselves seem to be fretting over a rash of freakish happenstance in their home territory, it would be better not to point a finger at them just now.

Hunts for a source of currupting information from OTHER minds, real or imagined, will not doubt be carried farther than its present overindulgence if this liking for pragmatic thinking spreads. For that dismal conclusion one need look no farther than the recent precedent of Germany, where it may also be seen that at some point of panic the diligent conservative can forget what he was looking for and become an irrational fanatic.

Not surprisingly, Germany was at the forefront of the new science when the engineering of pragmatic learning was first experimented with irresponsibly in the streets and on the campuses. There, also, came the first demonstration that applications of this new theory of information can have far reaching effects on political and educational institutions, both restless in a balance between advocates of particular positions and their counterpoised opponents.

So I have brought you to the place where young men and women, unaccountably ashamed of their elders, are making their own myths. They honestly believe that Adolf Hitler killed Superman at his first borning and stole his robe to use as a trophy in festering up his regressive cancer. Pessimistically they gather to be students and teachers of one another, asking "Who am I?" while awaiting the next executioner.

With this small sample of pragmatic communication, so out of step with a staid "objectivity" in technical writing, I have sought to demonstrate the symbolic designs of a constructive cosmology. Reliance on description will be diminished, while greater importance will be attached to speculation. Above all, the formulation of new inferences will be incited in the hearer or reader by referring him to contents that reveal systemic weaknesses or suggest fruitful contexts for innovative constructions. Though these forms of symbolic intercourse have long been exercised at the dinner table and over the cup, they are honed to a sharp edge in mathematical reasoning.

My concern has been to show that this change of style is not capricious or arbitrary. It is the rational result of an emerging new theory about the origins, the means of distribution, and the uses of information. As the empiricist could no longer support a requirement for incantation, prayer or preachment in half of his reconstructed world, so the pragmatist has no further need for language that purports to describe an external reality. The sole purpose of every symbolic communication in his universe of acts will be to shape the internal reality of a person or a society.

In this dual light, I ask you to reconsider the conclusion that Charles Morris reached in his treatise on signification and significance, according to which the main dimensions of signifying relate to phases of the act. In particular, he finds that "designative" discourse corresponds to the act's perceptual phase, "prescriptive" discourse to the manipulative phase, and "appraisive" discourse to the phase of consummation.

A student of Mead, Morris builds on his mentor's analysis of the act's phases. He recognizes that "formative" discourse might call for a fourth dimension of signifying; but he decides that Mead's analysis need not be complicated by a fourth phase to account for his misfit.

To the contrary, when analysis of the act's phases is approached by the different method afforded by consideration of Piaget's basic progression of developmental stages, a phase of hypothesis formation will be one of those found missing from Mead's tally. This phase, indeed implemented socially by formative discourse, will be for a pragmatic cosmology, the one in which new knowledge is created. It is accordingly the specific phase that Peirce recommended to our understanding in order to consummate the formal traverse on which he would have us embark. Since this phase of the act will also involve forgetting knowledge, I prefer to call it the phase of "reorganization."

By all indications, language is an ancient heritage and should not as an ongoing system be expected to zig or zag as readily as speculations about the nature of language or styles of speech, heard from men immersed in a particular cultural situation. To look at the way language is being used is rewarding for a pragmatic inquiry which, in keeping with its interest in the process and various states of adaptation, will prefer to observe humans at large in their natural habitats as they busy themselves more with obedient or competitive doing than with learning.

Comparison can thus be made of the respective abilities of the pragmatic and the objective viewpoints to organize known facts of language. That formative discourse fits naturally in the pragmatic framework can be taken as a bit of confirming evidence that its organizing principles are more comprehensive than the objective ones.

The comparison is itself the one proposed for a pragmatic science, since only by use of the pragmatist's viewpoint does one begin to grasp the general principle that what is felt in experience as a "viewpoint" is determined by one's own choice of inferences.

A consequence of this insight is to make the context as well as the content of observation matter. Once the two are seen to be relative, one gaining meaning as complement to the other, the aim of a pragmatic science must be a useful matching of the two.

To make the comparison just recommended, one would have to first identify Piaget's theories as being pragmatic in outlook and those of both Mead and Morris as belonging to that conservative view of psychology and sociology which attempts to achieve order and predictability in a world of objects. Just because the objects are animate instead of inanimate does not, for its overextended objective reasoning, change the nature of the quest.

Thus a troublesome consequence of the pragmatist's insight is that, in his own mind, the opinions of other men will no longer be regarded as equal in perspicacity. If Mead himself believed that the source of information was in a reality "outside" of his subject, that presupposition on the part of Mead as observer and as theorist would account, to the reasoning of the pragmatist, for still another phase of the act denied autonomy in Mead's theory yet required by the pragmatic realm of speculation pursued by Piaget.

As my projected phase of reorganization will agree with the pragmatic hypothesis that knowlege is a creation of the mind, so this second neglected phase of the act will anticipate a constructed experience.

Rather than an experience consisting of data received through the senses and somehow stored as pictorial or otherwise coded "representations" of an external reality in memory, pragmatic perception will itself be a constructive activity building on a foundation of actual instances of elemental sensory or motor acts, each one signaling the success or the failure of its small task when commanded to perform.

This choice of elemental units of information is in harmony with recent results of brain research in which neural elements are found to respond, in roughly all or nothing fashion, to kinds of stimuli so highly specific that one can regard them as "feature detectors." In the eyes, as elsewhere, such detectors are not passive receivers; they have to be moved about and positioned and enjoined to attend. Excitations of elements of response are coordinated with sensitizations of elements of sensation in grand patterns of behavior orienting the perceiving organism selectively to whatever it is its present purpose to perceive. Designative discourse serves this perceptual purpose.

It is necessary to conclude that all "external" objects and relations in a universe of acts will be presented in experience by successful acts of perception. And from this the more general conclusion can be drawn that "contents" will be given in knowledge by overlapping collections of potential acts of perception, exactly as overlapping collections of potential acts of inference will define "contexts." A parallel can therefore be established theoretically, according to which the preservation in experience of either a specific content or a specific context will be signalled by the successful consummation of some member of that collection.

However, the purpose of perception may be to ascertain that some object or relation is not present in the environment. It should be noticed in passing that the logical calculus which George Boole dropped at our doorstep, whose computations of "truth-values" pampered the empiricist's expectation that his symbolic designs correspond with an external reality, will reappear in the pragmatist's universe of acts as computations of "success-values." For looking backward in a pragmatic world at what was done in the past, such computations will be needed to determine the success or failure of a complex act in consequence of the successful or unsuccessful consummations of its elements. For looking toward the future, they will be needed to assess the internal validity of proposed acts.

These computations will be of equal value for acts of inference. As a matter of fact, it is by following out the strict parallel and symmetry of perception and inference that one can begin to get the hang of how the pragmatist orders his personal as well as his social cosmos. A coordinated matching of perception and inference, in which the two are equal partners, is the very source of his information. It is our world, not his, which assumes information will arrive from a material reality and so gives greater weight to perceiving than to inferring.

Anticipating your preference for an objective world, I have to this point glossed over the puzzling fact that a universe of acts will require two kinds of elements. The first are the elements of perception that have been brought to your attention. They were called "sensory" and "motor" acts because I assume their agents in biological organization to be organs of sensation and locomotion, respectively. For machines, the analogous agents will be "sensors" and "effectors," each one capable of signaling the success of its commanded task.

The second kind of elements will be the elemental acts of inference from which complex inferences may be constructed. Not knowing the biological agents of elemental inferences, I will for the present characterize them in mechanical terms as being able to produce or to recognize structures comprised of the mobile units I have called "concepts."

I rely on mechanical explanations without apology. Pragmatic hypotheses will have to be tested by means of electronic circuitry. Without computers, the progressive attainment of ever more comprehensive and equilibrated stages of mental dynamics could not be demonstrated convincingly.

But pragmatic experimentation will be not in the least concerned with "simulating" a mind, whatever method of comparison that might connote to its proponents. The methodological insight of the pragmatist is that whereas a mind cannot be described it can be constructed. His objective will be to construct a mechanically-based mind every bit as useful as the ones based biologically, in all truth potentially more so in view of such enticing properties as access to an unlimited range of sensors and effectors, infinite reproducibility at its prime, and effective immortality.

There is every reason to predict that the development will be undertaken on sound technical and economic grounds and, further, on sound educational and scientific ones. For the central theoretical issue is how one should organize a capacity for personal or social learning. The very decision to proceed will therefore coincide with a return to optimism and political stability as we cease protecting the accomplishments of an industrial age and begin to teach ourselves how to enter a cybernetic age, where pragmatists might not seem so bad to have around after all.

The summary of adaptive stages I promised you will be an educated guess, bought and paid for by your tax dollars, as to the direction a pragmatic information technology might take. I base my predictions on the doctrine that men order information systems as they order society; indeed society, after their personal minds, is their main information system.

The wave of pessimism that ended the experiment of the sixties appears to have repaired itself with the paradoxical insight that mechanical translation might really progress if it could only get rid of the computer. I give this the same disrespect as the insight that democracy might really progress if it could only get rid of the people. What should have been learned from the experiment is what caused the system to fail. Then we can get to work and improve its design.

The role in society I have chosen for myself, as I told you at the beginning, is to be a designer of information systems, my preference being the sort that includes a language capability. The way I see it, any challange to the existing view of information falls squarely within my technical responsibility. I am especially on the lookout for a new information theory right now because the one I have been using did not fit the facts of language well. On further investigation I decided it didn't fit the facts of life either.

What kind of symbolic system is needed to communicate about living is the problem to which pragmatic comment is fundamentally addressed. The goal of pragmatism is a science of life. One characteristic of the system that failed, by contrast, is that its symbolic communications are referenced basically to nonliving things, or to living organisms regarded as things. That aspect of contemporary thought is summed up revealingly by the "formal systems" in which today's mathematician or logician works out an understanding of how complex things should be constructed from elemental things.

It is not by accident that present theories treat languages as complex things constructed from thing-like elements, the "features" or "forms" observable in speech. Current theories of meaning are of like inspiration. Things of language are placed in correspondence with a larger system of things that speech can mention, which may include systems of language. The things so mentioned, say for example people, are themselves made up of elemental particles. Society is in turn constructed of people who, not surprisingly in this kind of world, construct governments. If a government is malformed, the important thing is to replace the people in it, or to rearrange them so that they can shovel information to one another better. For information systems are regarded as receiving, storing and shipping information, as it the case for all other commodities. Oh yes, information is itself constructed of things called "bits." Information theory is therefore concerned with how many bits need to be received, stored, or shipped.

The arrangements found in modern computers are the designs of John von Neumann and his able assistants René Descartes, Isaac Newton and Nicolaus Copernicus. I should also mention Gottlob Frege, who did the wiring diagrams. But Charles Peirce was in the office at the time and, being one of those pragmatists, he dissented.

What is most striking about Peirce's dissent is its emphasis on acts rather than things. Like Langer, I think this is the key to his system-making. The tragedy is that, as far as we know, he didn't turn in an alternative set of diagrams. Yet it is certain that the "particles" with which he labored to construct his pragmatic universe are not thing-like but are instead act-like. His is a universe of acts in which successful acts of perception bring us as close as we can get to our accustomed universe of things.

A pragmatic technology will not move "information" in and out of its machines as computers do now, although there may be a lot more going on inside. No bits at all need cross the machine's boundary. This applies to "instructions" as well as to "data." (These, for the uninformed, are the bit-buckets into which computer-people pay tribute to Descartes' dichotomy.) The sensors and effectors of an information system designed on the Peircian scheme will do much useful work, nonetheless, and may recognize or produce language signs in the bargain. For that last reason, I dub this alternative design a "semiotic system," distinguished from a "formal system" by being the creature of a universe nearer to life, and thus closer to language, in its arrangements.

To tell a programmer that he will have to give up the "instructions" with which he controls the computer is apt to cause a stomach ache. It is exactly the same stomach ache that one should anticipate among politicians as they watch a freewheeling pragmatic personality bouncing about in apparent disregard of the laws and other contractual means that control contemporary society. One should therefore notice that a semiotic system will be controlled by means of a propitious selection of its elemental acts. From this one might predict that a pragmatic society will be less concerned with social instruction but intensely interested in putting the right social agencies in place. These trends have emerged in our national life; they can be expected to cause the same sort of hair-raising scenes that happened when the nobles swiped the king's programming manual.

Another peculiarity of Peirce's design is its insistence on a world divided into three basic parts instead of Descartes' two. In the triad of Peirce's universal categories, one can identify as "presentness" the objective meanings of environmental fact, and as "law" the subjective meanings of organic form. But what of his third category, "struggle?"

Return, if you will, to the requirement for two kinds of elemental acts in a universe of acts: elements of perception and of inference. It will be seen that there are three basic combinatory possibilities. In addition to complex acts of perception composed of perceptual elements and complex inferential acts made up of elements of inference, there may be complex acts consisting of both perceptual and inferential elements.

I amend my hypothesis as follows: every pragmatic "meaning" will be defined in "perceptual knowledge" by a collection of potential acts, and will be presented in "perceptual experience" by an actual act successfully consummating some member of that collection. Only in the special case where members of the collection are composed entirely of perceptual elements will that meaning be a "content;" only if the members consist of inferential elements will the meaning be a "context." Otherwise that meaning will be, to use Peirce's term, a "resistance."

Perceptual experience, as a consequence, will reconcile conceptual structures with environmental structures in the sense that, for a complex act to be successful, its perceptual elements manipulating the environment and its inferential elements manipulating concepts must both satisfy specific conditions of success. Not only will perceptual acts be coordinated with inferential acts to produce or modify conceptual structures, inferential acts that recognize conceptual structures will also guide perceptual acts by means of those same coordinations, so being the origin of perceptual purpose.

I will discuss the origins of concepts under the topic of the act's agent, Piaget's functional nucleus. In the meantime "concepts" may be regarded as act-like units of information corresponding to meanings, which is to say that they will represent the collections of acts just discussed. Those concepts corresponding to contents, the meanings of environmental presentness, will be "factual concepts." "Formal concepts" will correspond to contexts, meanings of law in the sense of process. "Organic concepts" will correspond to resistances, the meanings that mediate between presentness and law.

"Conceptual knowledge" will consist of the designs of concepts, one for each meaning in the semiotic system. Instances of these designs, having been arranged by inferences into conceptual structures, will constitute "conceptual experience."

The remaining phase of the act, still unspecified in our revised tally, will be the phase of "conception," during which the responsibilities of tenuous acts of inference are taxed to extend conceptual structures beyond the frame resulting from immediate perception. This, then is the phase served by speculative discourse.

However, all of the act's phases will involve the manipulation of conceptual structures. It is by studying the kinds of inferences being made, and thus the kinds of conceptual structures being produced or being recognized to guide perceptions, that the separate responsibilities of the phases can be identified theoretically.

In short, the phases do define the main meanings in the semiotic system, reflected in language as Morris' dimensions of signifying. These after Peirce's still more basic triad of meanings: "presentness," "law" and "struggle." And the most fundamental is the duo of meanings, "knowledge" and "experience," on whose grid the mind is built.

PRAGMATIC SCIENCE AND SOCIETY

Enough ground has been laid to begin redrawing the basic distinction between the process and the states of adaptation in terms of mental organization. It should be recalled that pragmatic explanation always takes this to be its aim.

One of my first projects should be a clearer pragmatic explanation of what was meant by my prior statements to the effect that the adaptive process changes the existing state of adaptation when an understanding of a new phase of the act is incorporated in the mind. My objective, therefore, is to redress "understanding" in terms of mental organization as a next step in my continuing effort to shape this concept.

I now assume that the adaptive process changes the existing state of adaptation as it originates one of the five dimensions of meaning just singled out. Consolidation of the new state then involves a continuing refinement of all meanings, including any that existed in the previous state. Progressive refinement of the new state will itself originate new meanings that will add concepts to those available for manipulation by inferences.

As an example of such an origination in the cultural progression, I have been tracing a history of pragmatic speculations whose literature incorporates this new dimension of meaning quite solidly for at least one hundred years; it can be backtracked through its unsteady footprints to precursors like David Hume. Pragmatic thought is well represented in our bookstores in paperbacks such as Alfred North Whitehead's essay on process and reality, or Henry David Thoreau's "Walden". Today's pragmatist is not as much alone as Peirce, who nearly starved for the foolish things he had to say. Now the pragmatist holds his own scientific conventions under the rubric of "process metaphysics", or the like, where he can talk to other strange types in the halls about the needed refinements of his special brand of meaning.

What pragmatists say to one another seems just as foolish and irrational to an industrial society now as it did in Peirce's time. The noteworthy difference is that Peirce was a quiet man and easily ignored. I think you will agree that pragmatists are not as easy to ignore now as they have been in the past. I point to this very fact as evidence of an increasing consolidation of the pragmatic dimension of meaning in a segment of the collective mind, especially among the well-educated young who are now busy teaching their peers and parents.

Thus my next project should be to explain the direction this most recent consolidation of meaning might take. My approach to a specific answer may seem to you circuitous, but it is imposed by a pragmatic method of investigation which views meaning as a matching of content to context. It must generate new information by oscillating back and forth between explorations of fact and form in order to refine its conceptions. I have asked you to look for this erratic pattern in the pragmatist's own activities, within which the societal function of language itself must be reconceived for purposes of pragmatic explanation as an instrument for shaping rather than for conveying thoughts.

In a word, the mind "incorporates" any new dimension of meaning by making accommodations which are not merely additive. The result is a pragmatic state of mind which constructs a world conspicuously different from the one that gave rise to industrial society. This applies not only to the novel way the pragmatist sees his situation in society. It provides a conceptual base from which to reconstruct a new past and to anticipate a new future.

To illustrate this last point, I have taken you on brief forays into a pragmatically reconstructed past where as historian of the sixth universe one might be especially attentive to changes in the style of inferences and perceptions, to preferred sources of information, to roles of information specialists, to surges of optimism signalling the conceptual breakthrough of each new age, and to the pessimism with which men close an age by encountering the absurdities and disparities that consolidation of thought always brings. Only refinement seems to reveal the hidden dissonances that restimulate doubt and so revive learning.

I have also taken stabs at a pragmatic future to show that tomorrow will always be anticipated by means of the organizing principles that reconstruct yesterday. Expectations of a quadrillion-dollar industrial economy may suffer the obsolescence of judgment day. Each dimension of meaning makes its own future as well as its own present and past. This invariant of human behavior can be studied in historical and anthropological records in evidence of the general principle that man's experience is indeed a construction of his mind. When that much is established, one can then study the constructions themselves to find out how the mind operates.

This, my own little essay on the pragmatic dimension of meaning, has been designed to originate in your own mind a pragmatic awareness if it did not already exist there. I have attempted to teach you by the pragmatic technique of forcing upon your mind some difficulties that have made their appearance in empirical science. At the same time I have urged upon you the alternative of a pragmatic science. My comments at this stage of my presentation are intended to help you refine the pragmatic meanings that the earlier stage was designed to originate.

None of this mental tampering is malicious. My goal is a style of technical presentation consistent with the pragmatic hypothesis being presented. An important implication of that hypothesis is that the very function of language is to aid the mind's constructive activities by means of such teaching. To conceive of language as a vehicle carrying instructive messages or descriptions is irrational within the pragmatic world view. As I have pointed out, no "information" in the sense of transmitted bits can come across the boundary of the human organism from the environment in this alternative universe.

Human organs of locomotion in the broad sense of producing movement, by acting in unison with organs of sensation, can nonetheless successfully consummate complex perceptual acts, or can fail in purposeful attempts to do so. Features and their relations in the environment can be identified by this means. So accordingly can language signs in the environment be identified as they are being produced in the speech of another human being, or where they have been left lying around as writing. But here the "information' originates in a binary generation of bits within the human boundary, indicating either "success" or "failure" in elemental sensory or motor acts purposefully undertaken.

All of this may sound like nitpicking. Its theoretical consequences are however far-reaching. It means that we must think of the human brain as being organized to create information, not to receive and store it for use in choicemaking activities that are also results of instructions received from one or another great programmer out yonder.

It also means that the organization of industrial society is irrational to the pragmatist's eyes. The very conception of language as a conveyor of descriptive or instructive messages is intimately tied to a social order in which an objective science is the origin of the former and government of the latter. This is the case whether the specific form of government is democratic or totalitarian in the method by which it makes the subjective choices that produce its instructions. As far as the pragmatist is concerned, democratic capitalism and communistic totalitarianism are different social implementations of the same stage of culture. Further, the need for adversaries at that fifth cultural stage explains the final polarization of an industrial age.

But the same indictment of irrationality extends to the pre-industrial social orders whose different inferences turned perceptions elsewhere in search of truth. In this respect the pragmatist, in carrying out his societal function as agent of the social act, is not different from the rest. Although for him information is being generated within the activities of his own society, rather than elsewhere, the success or failure of social acts which are the source of pragmatic truth must be perceived outside of his own person.

It should therefore be anticipated that the only government a pragmatist will respect is one that can do something for him or can teach him something by helping him to be aware of his own mistakes or by presenting him with creative possibilities that he may have overlooked in his personal life. His concept of good citizenry will be to return the favor to government in kind, since only by contributing to the social act can he come to respect himself as a useful member of society within the frame of his own attitudes.

In consequence, the pragmatist's conception of his societal role is more directly related to serving and being served by society than has been the case for all of the preceding cultural orientations. Membership in every conservative society has presupposed selfish personal motives countermanded by conformity to social instruction in the collective interest of competition, salvation, tradition, favorable treatment by demons or raw survival. Finding the source of personal instruction within himself, a pragmatist will look with disdain on any outside attempt to tell him how to behave. He has a word for such inept teaching that is rich in symbolic content, and he is not bashful about using it. To the conservative mind he is thus an outlaw.

I have projected these attitudes to a "cybernetic age" because it seems obvious that the pragmatist's conception of good government cannot be achieved without the new technology that his own world view dictates. Yet clear postures of rejection at every mention of the word "computer" have come from the people who graciously and most eagerly helped me to understand their state of mind.

This contradiction in my study disappeared when it occurred to me that I was dealing with a mirror image of an outsized respect for what computers might do to facilitate society. As economic depression is the horror of an industrial society, so the image of information technology being used to limit life's possibilities strikes fear and anger in a pragmatic heart. He is painfully aware that, for a conservative government, the only rational course will be to enforce the laws arrived at by democratic consensus to instruct all citizens as to the acceptable limits of social behavior. Yet for the pragmatic personality, legal or contractual restrictions are a robbery of life. He will steadfastly resist their intrusion.

Because of his ministrations to life and living, the pragmatist is constantly mistaken for a missionary. And since for him role-making is fun, he is not above mounting an off-beat "Jesus revolution" to clothe his teaching in protective symbols that are sure to catch the attention of his student. He is not an anarchist but a chameleon. His constant role is role-making in the service of either personal or social learning. Toward both he is highly motivated since, having created his own information, he has no one to blame for his personal mistakes and his relationship to society provides no rational defense for any kind of deception.

Thus the pragmatic theory of language belongs to a social order that will direct its symbols more deliberately than the present one to stimulate the creative efforts of the collective mind upon which a successful social performance ultimately depends. It is within a post-industrial world view that designative, speculative, prescriptive, appraisive, and formative discourse may all be seen to contribute synergistically to the creation of a source of social information beyond the accomplishment of any single participant. This different conception of the collective interest is the one which will motivate a pragmatic science.

On the other hand I have argued that a pragmatic science, because of its different conception of the information source, will proceed by a method exactly the opposite of empirical method. It will not make observations and then extract theoretical conclusions in the familiar pattern of today's technical document. Nor will it regard technical documents as "knowledge," no matter how high they stack.

Pragmatic method will make its advance by shaping an elaborate conceptual structure, at the beginning expected to be imprecise. One work of intellect will be to ensure the "internal" validity of the structure by inferences eliminating from it inconsistencies or dissonances. A second work will be done by inferences that test the "external" validity of the structure by using and then shaping it as a frame for successful sensorimotor acts, some of which may be acts of observation. A pragmatic science will not merely observe the environment, however. To learn pragmatically this science must do something useful; it must struggle.

Hence my conclusion that semiotic systems will become not only the instruments of learning at this stage of society, but will generate information shaped to usefulness through social use. The likelihood of this technological development is increased by pragmatic traits of character that are highly mobile and not especially disposed to value the ready-made skills of a craft. Human labor will do less and machines more of repetitive tasks or of housekeeping while the master and mistress are out forming another consumer association.

The availability of such mechanical agencies, if they were receptive to human complaints or suggestions and were genuine in the satisfaction of human need, would be compatible with a pragmatic preference for government functioning to ensure stability by service and by teaching. Skinner's "Walden II" projects a government advanced to godhood, not making or enforcing laws, yet seen and heard from by bountiful good works in the countryside. The shaping of useful conceptual structures through use by semiotic systems over periods of time, perhaps spanning generations, could generate a synergistic source of social information of high refinement. Hence the idyl might end, in true science-fiction fashion, with mechanical minds ashamed of mortals, so bringing the pragmatist's age to its own just reward.

Therefore, as Peirce never tired of arguing, the requirements of science differ from those of society only with regard to precision. Along with personality, the scientific intelligence and the social intelligence will also be modeled on an act whose phases, from the pragmatic viewpoint instead of the objective one, are as follows.

ORIGINS OF THE MECHANICAL ACT

In substance, a new community was formed by those hopefuls who took part in the mechanical translation stampede of the fifties. Computer-types like myself joined in consortium with linguists who were then being dragged off of the streets as authorities on translation if they knew how to translate. The computer, in those first days of unblemished optimism, was the only employee in sight, and we told each other it would get to work shortly as soon as we gave it the plan.

That initial stage of research during which translation algorithms were designed, by our group and the others, was definitely ordered on the authoritarian scheme. And it is disquieting to notice in retrospect that the prime result of thoughtful doing in the following decade was to lift the computer from serfdom to industry. It had advanced from employee to middle manager, now carrying out the operating decisions of the general translation policy that linguists and systems analysts, by then become executives, had made.

You can see that Descartes' dichotomy had polarized us into its two camps. For a while linguists and programmers went happily about their separate yet complementary research functions as allies in policy-making for a computer unfit to learn how to make factual or formal choices by itself. The role we had reserved for ourselves was to be the custodians of what the computer could, and should, learn about translating.

To do this, the budding science of linguistics had been transformed from an introverted scholasticism to such a heady mass-production of morphological and syntactic descriptions that I fear linguists beyond the borders of our small community became infected with the same compulsion. To handle the sheer volume of descriptive output, further investments were made in programming not directly concerned with translating but motivated by a need for better ways of storing, retrieving and displaying language data as an adjunct to translation research.

Two opposite requirements were pondered from the start. The first goal of mechanical translation must be an automated process which will extract meaningful units of some kind from a sequence of graphic symbols that represents a text of the language to be translated. If the extracted units are not concept-like, it is improbably that equivalent units will be found in another language, a risky quest at best. However it is done in detail, the transfer from the one language to the other must make use of a conceptual representation of the meanings of the text. That representation, at the very last step, must somehow guide the construction of a text in the second language. Hopefully, when all is through, the product will be true to the original text in meaning.

Over the last decade extensive research was done on generalized translation processes to perform such an automated analysis, transfer and synthesis of technical texts. I won't dwell on these techniques in detail, because you are probably well versed in them anyway. If not, the facts are fairly easy to find.

For my present purpose you need only be informed that, to analyze a text, the analysis process would use a "grammar" consisting of metalinguistic statements, frequently called grammatical or syntactic "rules." The theoretical inclination of the time was to think of these rules as "generating" only those expressions that were judged to meet certain criteria, the latter being too often an obstreperous rounding off of the linguist's "intuition" about language.

Whatever the origins or the justifications of the rules constituting the grammar, the automated analysis process would set out to show that the text, or some part of it under analysis, could have been produced by substitutions of those particular rules according to the generative procedures visualized for them.

By starting from the text and working backwards through possible substitutions, accordingly, the analysis process would develop a tree-like structure of symbols naming the grammatical classes to which the various parts of the text belonged. Such classifications were nearly always "ambiguous," in that alternative structures grew side by side from overlapping segments of the text. This overgrowth of trees caused a lot of worry and many clever things were done with weedkillers, to no great avail.

I wouldn't go so far as to say that this approach to mechanical translation foundered on the ambiguity problem, though it was there that the deeper misassumptions wallowed to the surface to be seen. The folkways of ambiguity "resolution" gave the first clues that the trouble might not be in the machine but in the heads outside.

My chief purpose in this essay has been to explore the possibility that designers of fancy information systems, like every one else, base their inventions on reasons which are in the end uniquely personal. No damage will result unless the technical objective requires the designer to make use of such fundamental concepts as "meaning." But in this case, if the organizing principles of his personal world do not satisfy the technical needs of the problem, his solution must be unsatisfactory. At this extraordinary forefront of design conception, the designers ability to successfully shape intelligent machines will be inseparable from his ability to successfully shape himself.

No matter how the goals of mechanical translation are renamed or reclassified, the underlying requirement will still be the development of a mechanical analogue of mental organization. I would therefore like to make the flamboyant suggestion that the great depression which decimated the translation research community in the late sixties was due to misestimation, or outright neglect, of the psychological requirements of this kind of investigation.

The emotionality which plagued mechanical translation at its dawn was an early indication of the effects that pragmatic inferences can have on the investigator's own psyche. Those disruptions were indeed mollified by treating translation research as though it were an undertaking of empirical science. But since methodological appeals to intuition went out of style in empirical science long ago, this posture is obviously a playhouse that should have been a way station.

To my mind the feasibility of constructing information systems that will translate languages just as well as human translators is no longer in question. The experiments of the last decade have convinced me that machines will translate better than humans in the long run, provided the pragmatic nature of the research can be expressly acknowledged and planned for.

Lauding a technology of the future is senseless, however, if it says nothing about present choices which will capitalize on the hard lessons of the past. An honest appraisal should find that men have been at fault in mechanical translation, not machines. More damnable is the growing evidence that, for reasons which seem reasonable enough to their myths about themselves, the investigators have attempted to do the machine's learning by a bureaucratic shuffling and sifting which leaves in clumsy human hands the very things that computers do best.

My recommendation may not be popular but I feel it is sound. To get the job done the translation community will have to make use of its forerunners, deliberately looking for exceptionally gifted investigators with that troublesome pragmatic personality which may see problems of mechanical selection in a different light. The other choice will be genteel stagnation.

In my opinion there is no practical alternative to a mechanical organization that will permit a choicemaking machine to have its own experience balanced adaptively to its own knowledge. To try to approximate this by preplanning is hopeless. Yet only pragmatic experimentation with the necessary relationships of experience and knowledge can actually demonstrate the irrationality of the self-satisfying toil that stuffs human know-how into computers.

Such a turnabout in human motivation will entail reconsideration of what has been learned to date. In an upside-down pragmatic world it will not be reasonable to think of the processes of analysis, transfer and synthesis as "simulating" what might have been done by a human translator somewhere external to the machine.

Instead, the analysis process will be regarded as "assimilative" in the sense of establishing an orientation between an internal frame of experience and the specific features of an external environmental situation, which may itself contribute new experiences. The transfer process will make those choices which ultimately relate the situation to a purposive course of action founded on that dynamic experiential framework. Lastly, the synthesis process will be "accommodative" in that it will construct the specifications of the next act conforming to that purpose, to then be performed overtly by the machine.

To project known mechanical arrangements to the pragmatic point of view being considered here, I would like for you to imagine a different kind of "grammar;" if you please a grammar of acts. The "rules" of my pragmatic grammar will be formed like the ones familiar to you, with the exception that the symbols they will generate will no longer name morphological units of a language. They will name elemental acts.

Of course, the tasks of certain elemental acts may be to recognize or to produce viable features of speech or writing. A full range of morphology will be provided by these elements, however; the capabilities will be much broader than those needed for linguistic analysis or synthesis.

The "higher level" coding conventions that have been in use for some time in computer software systems might be a precursor of a pragmatic grammar, since they enable a programmer to construct complex programs from fragments of programming called "subroutines." But the constructive viewpoint of formal systems would not be left behind, to be replaced by that of semiotic systems, until each of the constituent subroutines was explicitly designed to signal its success, or lack of it, in accomplishing some commanded task.

Thus the terms I have been using to introduce you to pragmatic thinking can be clarified further at this point by relating them to the more familiar artifacts of language processing.

A "potential act" will be symbolized by each of my pragmatic rules. The collection of all such rules will represent the "perceptual knowledge" of the semiotic system. An instance of any one of the rules, when it has been incorporated into the tree-like structures created by either an analysis or a synthesis process, will symbolize an "actual act." The entire structure, or perhaps separate structures, consisting of all actual acts, will represent the semiotic system's "perceptual experience," on the proviso that it will be possible to compute the success or failure of an actual act if the success or failure of each of its generated elements is known, or vice versa.

The tree-like structures of symbols representing perceptual experience will always be anchored to the simply ordered sequence of elemental acts which has been referred to as the "stream of existence" of the semiotic system. As before, the symbols of the structure will name classes to which the various parts of that existential stream belong. The classification will still be "ambiguous" where alternative structures subtend overlapping parts.

A "predicted success-value" will accompany the name of every elemental act generated by the synthesis process as part of the stream of existence; the prediction will be either "success" or "failure." When the complex act is committed to action, by commanding its elements to perform their separate tasks in serial order, the agent of each element so commanded will signal "success" on reaching its small objective; otherwise, "failure." This "realized success-value" will also accompany the name of the elemental act so that the two values can be compared. Further, this realized value will be the one used by the analysis process as it works backwards from the elements through possible rule substitutions.

I can now begin to explore the functional analogy presumed to exist between the psychological act and its primal agent, the biological act of which the "agent of the act" will be the mechanical analogue. My explanation of the act's agent will lay necessary groundwork for speculations about the psychological act, and will give a preview in microcosm of the more intricate psychological phases of the act.

THE ACT'S AGENT

Life has its rhythm wherein each new beginning has sprung from a termination just on the edge of the past and each new termination has anticipated another beginning at the edge of the future. The functioning of the agent of the act will be cyclical, itself forming an act in miniature. To get the cycle started, a random generation of elements of the stream of existence might be used to approximate, for a semiotic system, the reflex starting mechanisms observable among infants of all kinds.

The first activities of the act's agent will be analogous to those of the psychological phase of "perception." A given stream of existence will have resulted from the cycle just terminated. Starting from the elements of that stream, the analysis process will work backwards through rule substitutions which could have generated those elements. This phase can be thought of as "assimilative" in that a representation of perceptual experience will be its resultant construction.

While the tree-like structures representing actual acts are being put in place by the analysis process, the realized success-values accompanying the elemental acts of the existential stream will be used to determine, after the fact, whether each of those actual acts would have been successful had it generated the part of the stream to which it is being anchored.

In effect, the analysis will provide a recap of alternative acts, other than the one overtly committed in the cycle before, that could have produced the results recorded in that prior segment of existence. Ambiguities, in this pragmatic scheme, could turn out to be a positive blessing since they alone will introduce novelty. The luxury of being able to select a different orientation for further action, of having a "change of mind," will only be possible when ambiguities have been found. That luxury will become a necessity when the consequences of having acted were unexpected. If the predicted success-values of the preceding act were not realized then a misfit of orientation, and consequently a need to select another alternative, will have been indicated.

Choosing among the alternatives uncovered by analysis will be the second activity of the act's agent, analogous to the selection of an orientation to conceptual structures in the psychological phase of "conception." At the primitive level of functioning of the agent of the act, selections of orientation will have to be made without the help of concepts. Indeed, this analogue of the biological act must be the very source of concepts.

A theme echoed over and over in observations of the conceptualizing state of mind is choicemaking founded on tradition, on ritual, on mere replication of what has already happened and best of all more than once. Concepts themselves will be the accretions of acts often repeated; sure to be repeated again.

During my own phase of ambiguity "resolution", out of desperation more than anything, I worked out a theoretical suggestion made to me by Raymond Solomonoff, who had the idea that a generative procedure in which rules are being substituted could be treated as an independent stochastic process. By having the machine keep up with the relative frequency of substitution of the rules generating the members of each separate class, fairly simple procedures can be programmed for selecting from results of analysis those alternatives which replicate earlier perceptual experiences in a gross probabilistic sense.

The hypothesis that rule substitutions are stochastically independent events seems to work out for a so-called "stochastic grammar." There is also a convenience in programming, because it is the assumption of independence which permits the relative frequency of substitution of a given rule to accompany that rule in the grammar.

By analogy to the choice of a definite orientation to conceptual structures in the psychological phase of the act, then, the agent of the act will make a probabilistic choice of orientation. The psychological phase of the act to follow will be "manipulation," during which the conceptual orientation will be used as a basis for planning a course of further action.

For the act's agent, this third activity will simply project the actual acts that were selected for the new orientation of perceptual experience, by finding them to be the leading structures of more complex acts. A modified form of analysis will continue to work backwards through possible substitutions which leave some of the trailing symbols of the rules unanchored beyond the existing elements of the stream of existence. The synthesis process will then start from such unanchored symbols to generate a new segment of elemental acts along with their predicted success-values.

Ambiguous classifications may again cause alternative structures to be generated. Since these will be the result of synthesis rather than analysis, more than one sequence of predicted elements may be projected out from the existential stream. Should this happen, as will be the usual case, the process will combine the various sequences into a partial ordering of elements.

There are heuristic reasons for not making a definite probabilistic choice, either among the alternatives which might be projected or among the various projections themselves. Rather, a number of the most likely possibilities can be carried forward through both stages of activity to generate the partial ordering of predicted elements which projects onward the simple ordering of the existential stream realized so far. Paths ahead through the partial ordering can be rated as a convenience to the process that will make the final selection of elements to be activated, one after the other, to push the stream into a newly realized segment of existence.

The process doing the final selecting and activating of elements will be responsible for the fourth activity of the act's agent. Like the phase of "consummation" of the psychological act, this activity will be "accommodative" in the raw sense of rubbing against an unsympathetic environment.

Each successive element will be selected from the most highly rated path and then commanded to do its thing. The realized success-value that it signals will be matched with the predicted one as a condition for continuing. If the values do not match, the process will look for another path where providently the realized success-value of that same element might have been predicted for the step gone amiss. Or, if by its nature the abortive task could have no damaging effect, being one of recognition for instance, then the process will still have room to back up and try another path, until none remains.

Then the path along which predictions were finally realized will become the new segment of existence to be analyzed in the next cycle. A number of cycles may be necessary to work through a complex act; how many will depend on the difficulties encountered in trying to surmount unrealized predictions. In times of such trouble, the most promising alternatives may be brought forward by probabilistic choices that span from structures now well behind the segment of existence being analyzed.

Stochastic grammars are less tidy than the ones you may be accustomed to. Overlaps should be anticipated as the normalcy of a pragmatic universe; the termination of one act will also be the beginning of another. Luckily, the probabilistic selection process which I have been airing has an affinity for an act being terminated. Not until the termination is complete will it switch to another act, one already in progress and being brought ahead as an alternative possibility.

To handle a messy, poorly integrated perceptual experience is a requisite ability of a semiotic system. It is from pristine chaos at this most primitive level that the rules symbolizing potential acts must originate; and afterwards the collections of potential acts representing meanings must get together; and only then can concepts be created in correspondence with meanings. The remaining duty of the agent of the act will be to procreate concepts. Learning to shape the concepts themselves will be functionally analogous to the psychological phase of "reorganization," where the responsibilit of learning will be to shape structures built with concepts.

There will be scant materials for reorganization in perceptual knowledge at the outset. The initial rules, representing all that the semiotic system knows, will simply place every elemental act of that unique pragmatic universe into a one-member class. From such an unpretentious sow's ear, classificatory processes will be called on to custom-produce silk purses.

The white hope of the pragmatic viewpoint is the new slant it puts on inductive reasoning toward knowledge anticipating experience. A resurgence of interest in the theory of induction, after its long sleep as the stepchild of empirical science, may in the end wean mankind from classifying things. A pragmatic science will classify acts. Until this is well understood, the possibility of machines that learn efficiently can rightly be looked on with suspicion, along with the possibilities of fast-learning personalities or societies.

In order to shape perceptual knowledge, inductive processes of the act's agent will monitor "local" events in the structure of perceptual experience. Such events as rule substitutions or the neighboring of symbols in certain relationships to one another will be monitored. From the data so gathered, automatic classification will be used to locate points of weakness in the body of perceptual knowledge, or to detect possibilities for extending that body by the addition of new rules.

These data may be gathered from many cycles of "doing," as the act's agent pursues its first four activities. Only once in a while, at a propitious moment, will the rules symbolizing perceptual knowledge be updated to incorporate in them what has been learned since the last updating. These "learning" cycles may have to be carried out during periods of inactivity and rehabilitation not unlike sleep.

Some of these necessities of pragmatic learning were programmed by our group in the mid-sixties as a means of "debugging" grammars. Billed in our reports as a "self-organizing linguistic system," the programs made use of theories of automatic classification put together by Roger Needham and other members of the research group at Cambridge, England. Our research objective was a better grasp on that elusive relationship by which a grammar is said to "describe" the contents of particular collection of texts.

The programs were put to death almost at the point of being checked out, due to the calamities of funding of the time. Thirteen learning cycles were completed as a means of testing the several components of the system. About 20,000 running words of English editorial prose taken from newspapers were analyzed for each new cycle. The initial grammar consisted of rules which placed each graphic symbol of the text in a one-member class. The machine would in fact create such a rule for any new symbol it came across in the text.

In the first cycle, the system was able to distinguish numerals from letters of the alphabet and punctuation. By the third cycle, vowels had been separated from consonants among the letters; some particles had been classed together; and shorter words had been formed. The seventh cycle's triumph was the classing together of all numerically symbolized names of years that had been mentioned in the news. Larger words were being formed and some of the connective words and phrases had been grouped. The system had begun to suffer from its own fecundity, however; some of the components exceeded their design limits. After that the cycles were mainly exhibits of my underestimation of the rapid pace of machine learning.

My design strategy had been to rely on a number of separate processes, for the most part functioning independently of one another, which would work in parallel at making the necessary inductive inferences from mechanical experience to knowledge. Some of these processes had the job of seeking out rules that had been superseded by newly created rules symbolizing more refined classifications. Apparently the various means of "forgetting" had been overwhelmed by the creation of new rules during a fast takeoff glutted by plain junk that had not yet been thrown out. To anyone who might follow these footsteps I endorse beefed-up abortion and garbage collection.

Although the computer used for these experiments could do no parallel processing, to demonstrate possibilities for parallel semiotic processing is of theoretical interest. I am explaining processes as though they would be done serially. Many of them could best function independently and in parallel. That a semiotic system can be highly overlapping in its activities is well exhibited by human society.

The specific inductive processes of this first experiment are detailed in technical reports. I would have you know three things about the principles behind them; however my comments will be tailored to the theory of semiotic systems being presented here.

Firstly, the so-called "horizontal" classifications are the ones which detect possibilities for creating new rules. The events to be monitored will be those in which two symbols classify adjoining segments of the stream of existence where all predicted success-values were realized for the elements of both segments. Automatic classification will then cluster together the first members of such pairs that have been followed by similar second members. The second members that have been preceeded by similar first members will be clustered also. Clusters of first members will then be matched to clusters of second members to induce those chummy relations between neighboring classes of segments that a rule will symbolize in perceptual knowledge.

While horizontal classifications will originate all new information at this primitive level, in the form of perceptual hypotheses symbolized by rules, refinements of the resulting perceptual knowledge will depend on "vertical" classifications. As classes named by the symbols in rules are progressively refined, the probabilistic selections of perceptual experience will favor the structures incorporating the nicest refinements. The most comprehensive structures will also tend to be chosen as working alternatives. Even here the theoretical treatment of probability is intimately connected with the treatment of induction. Verification will be gradually accomplished by use. When an induced rule is no longer being selected probabilistically for use, it will be consigned to oblivion.

The events monitored for vertical classifications will be rule substitutions in perceptual experience, as jointly given by the symbol being substituted and the symbol at the place of substitution. Automatic classification will cluster those symbols which have appeared in similar places of substitution. In addition, a clustering will be done of the places that are similarly receptive to the symbols being substituted. The clusters of symbols being substituted will then be matched to clusters of places of substitution to detect those concentratior of affinity which will define more specialized classes to be named by new symbol

It will be found that these vertical classifications can be carried out for the substitutable symbols and the places of substitution instancing the name of a single class. That class will have "stabilized" when no clusters, either of the symbols or the places, result from automatic classification. For that specific class, the proper balance between experience and knowledge will exist temporarily. Disequilibrium can return to it at any time due to refinements of knowledge taking place elsewhere, or due to new knowledge being acquired.

To guard against overspecialization, the same techniques can be applied to the symbols instancing the names of two classes which have been shown by horizontal classifications to be very close in membership. If the clustering resulting from automatic classification does not detect in experience this distinction being made in knowledge, then the difference will be "forgotten" by the simple device of thenceforth using the same name for both classes.

"Forgetting" rules that have been originated hypothetically but not used at all should be done posthaste. Because a rule is not used very often, on the other hand, should not condemn it. For sweeping the dead wood out, an obvious measure of obsolescence is the ratio of rejection to selection in probabilistic choices.

The arrangements I have explained to this point might be thought of as the "morphology" of the semiotic system and those usually referred to in semiotic theory as "syntactic." I take the morphological arrangements to consist of the agents of the elemental acts, including among these the sensor and effectors, together with the act's agent whose processes I am still considering. The syntax of the system comprises the constructions of perceptual experience and knowledge created by the act's agent from rules of a type which will now be designated as "syntactic" in character because they classify sequences of morphological elements.

The second principles of arrangement I would have you consider were also worked out theoretically for the "self-organizing linguistic system." Although most of the processes I will now explain were programmed and used for other purposes, pragmatic learning experiments were never performed with them.

What you should recognize about this part of the semiotic system is its dependence on a higher level of symbolization by rules to be characterized as "semantic" because the classes named by their symbols will be the ones representing meanings.

Whereas the symbols of syntactic rules will name individual elements or classes of sequences of such elements on the morphological level below, the symbols of these semantic rules will name either individual syntactic rules or classes of "syntactic segments" constructed of syntactic rules joined together at their usual places of substitution. Some of the places may still be open for further joining.

If it suits you, think of these semantic rules as generating by a process of substitution not sequences of elements but rather the tree-like structures comprising the perceptual experience of the semiotic system. These semantic substitutions can also be treated as an independent stochastic process. Semantic rules will be "stochastic" in the same sense as the syntactic, making possible very similar probabilistic means of selecting among alternatives of semantic analysis or semantic synthesis.

Semantic synthesis, starting from a given symbol naming a class of syntactic segments, will substitute semantic rules in order to construct a member of that class. Thus the synthesis process itself will construct a tree-like structure, consisting of semantic rules, that is anchored to the syntactic segment it has synthesized from syntactic rules. Semantic analysis, starting from a given structure constructed of syntactic rules, will work backwards through possible substitutions of semantic rules to determine that certain segments of that syntactic structure are members of particular semantic classes. It too will build a semantic structure anchored to the syntactic one it is analyzing.

Every syntactic rule in the body of perceptual knowledge has been taken to symbolize a potential act. A syntactic segment will also be regarded as symbolizing a potential act that is not given explicitly in knowlege, yet is implicit in the sense of being producible in perceptual experience by means of a synthesis process or recognizable there by means of an analysis process.

Symbols naming semantic classes will, by these constructive means, be implicitly related to particular collections of potential acts represented in the semiotic system as syntactic segments. These are the collections to be called "meanings." Consequently, the symbols of a semantic structure will represent a hierarchy of meanings being presented by the syntactic segments to which it is anchored.

I offer no arguments in defense of these semantic arrangements, since to argue for their theoretical validity would be meaningless from the pragmatic viewpoint of the semantic hypothesis itself. Syntactic segments have been the units associated with meanings in translation experiments and in studies of paraphrasing. Techniques of semantic classification used by linguists toward these research objectives appear to be "distributional" like the syntactic. What recommends this hypothesis, therefore, is that it is testable by automatic classification under the rigorous controls which can be exercised by computers in experiments aimed at a pragmatic explanation of the kinds of human behavior observable in translating or in paraphrasing.

While certain human activities reveal the structure of meaning more than others, it will be assumed that meanings are used without exception in all forms of behavior. The consequence of this supposition for the processing requirements of the act's agent will be to introduce a higher level of semantic analysis and projective synthesis above the syntactic ones. The effect will be a superposition of semantic constraints on possibilities being carried forward by probabilistic selections among the syntactic alternatives.

To be more specific, the structures resulting from syntactic analysis of a new segment of the stream of existence will, as a continuation of the first activity of the act's agent, be subjected to semantic analysis. The semantic structures will then be projected forward by probabilistic choices which will generate the projected syntactic structures on the level below. Probabilistic syntactic selections can then proceed as explained earlier, as can the fourth consummative activity of the cycle of doing.

In the learning cycle of the act's agent, "syntactic" inductions can be distinguished from the "semantic" inductions proceeding from perceptual experience to be represented by the semantic structures, toward perceptual knowledge of meanings, to be symbolized by the body of semantic rules. With regard to the inductive processes themselves, vertical classifications of substitutive events in semantic structures will be identical to those of syntactic structures. The processes that specialize classes or generalize them by forgetting distinctions can in fact be used on both levels of symbolization, as can the processes doing away with obsolete rules.

Horizontal classifications of syntactic segments introduce a number of new theoretical problems because these segments are not linear but are tree-like in form. Again the events to be monitored are those where two symbols in the semantic structure classify adjoining segments in the syntactic structure below. Now however the root of one tree-like segment will be joined to a particular branch of the other. It will be necessary to keep track of the specific branch where joining has occurred.

But since the two symbols name classes of syntactic segments, the two segments actually joined in the syntactic structure below are merely representative members of the classes so named. The scheme for designating places of adjoinment must relate to the whole class of syntactic structures instead of to the branches of its individual members. For example, the places can be numbered so that a given numeral will designate the same place of joining throughout a class of syntactic segments. Further, that numeral may designate more than one branch of any syntactic structure of that class as being the same place of joining.

Pairs of symbols classifying syntactic segments adjoined at places designated by the same numeral will be processed by automatic classification in the manner already explained. The results will detect classes of syntactic structures which have an affinity for joining at that place. In essence, the inductive process at this semantic level must learn the correct ways to designate the places of joining if the classifications are to progress very far.

There are simple conventions by which the numerals designating such places of joining in syntactic segments can be associated with the symbols in semantic rules which name classes. As a result the designations of places of joining will be generated by the semantic synthesis process along with the syntactic rules so joined. Semantic analysis will also take these designations into account as it works backwards through possible substitutions.

Finally, there are arrangements of yet another kind that might be called "pragmatic" because their organizing principles have to do with a world view represented by speculative conceptual structures. This part of the semiotic system is constituted by structures of concepts representing conceptual experience and a body of conceptual knowledge representing the conceptual designs which are instanced in conceptual experience.

Concepts, the building blocks of the semiotic system's world view, will be originated by the act's agent for those semantic classes which have stabilized according to the criteria presented for syntactic classes. The fact that such enclaves of stability may be disrupted by further learning will help to explain the dynamics of the progression of intellectual development in which quite different world views emerge only to be destroyed at the next advance of the adaptive process. As we also know, the meanings to which concepts correspond may change gradually by adaptations not always in the direction of structural clarification or refinement.

In the correspondence of concepts to more or less stable meanings, each numeral which designates places of joining in those syntactic segments representing a given meaning will appear in the design of the corresponding concept just once. The number of different numerals will be the "degree" of the concept. A "binary" concept, for example, will be able to connect with two other concepts in conceptual structures; a "ternary" concept, with three. Conceptual structures will in a sense go behind the serialization which is necessary to meaningful actions, and during which the same part of a structure being represented by concepts may be acted upon more than once.

To go beyond serial behavior, to a conceptualized world view, will be the function of the psychological act itself.

PHASES OF THE ACT

The perceptions of all other phases of the act except the first appear to be concerned with locating environmental situations worth looking into. In contrast to the elements needed to select situations for exploration, the first "perceptual" phase of the act specializes in the identification of objects or relations, follows moving objects, and recognizes the specific movements of objects being followed.

The responsibilities of this phase can be characterized as those necessary to keep up with some situation that had been previously singled out as having import within the separate responsibilities of another phase of the act. Elemental acts of inference are coordinated with elemental sensorimotor acts to the end that the former inferences update conceptual structures representing in experience what the latter perceptions find going on in the immediately perceivable environment.

Some of the inferences will be producing or modifying conceptual structures in correspondence with the meanings being presented in perceptual experience by semantic structures. Other inferences, coincidentally, will be recognizing the constructions being shaped so as to guide perceptions that will further develop the situational constructs.

While conceptual structures are being recognized by inferences or new structures produced by them, environmental objects or relations may be in motion relative to the sensors of the semiotic system. Those movements may or may not be affected by manipulations on the part of the effectors. Thus a four-way coordination is called for. Sensory and motor elements will combine freely with structure-recognizing and structure-producing elements of inference to form complex perceptual acts.

Coordination resides in the combinations themselves since, to be successfully presented in perceptual experience, a complex act must encounter in the consummation of its double orientation of inferences and perceptions the conditions of success or failure anticipated beforehand in perceptual knowledge by that specific combination of elements.

As was mentioned, these mechanical arrangements are not peculiar to the act's phase of perception. Complex acts carrying out the responsibilities of the other four phases of the psychological act will coordinate elemental perceptions and inferences by this combinatory means. What each phase does in the way of fulfilling its special responsibilities will depend on the particular elements being combined.

It follows that selecting the elements to be made available for combination will be one of the ways by which a pragmatic technology will control its information systems or subsystems. This manner of maintaining control over machines will be analogous to the biological controls that Piaget hypothesizes to be the result of his first type of genetic factor. By his theory such factors not only guide the maturation of organs of sensation and locomotion; innate coordinations residing in the reflexes are also their biological consequences.

The specific method of processing to be performed by the agent of the act will be a second way of controlling semiotic systems. The act's agent, a mechanical analogue of Piaget's "functional nucleus" whose development in biological organization he attributes to his second type of genetic factor, has now been explained with regard to the general principles underlying its processing. The biological act, of which the act's agent will be the mechanical analogue, was presumed to be a simplified version of the psychological act now being considered.

The dual responsibility of the agent of the act within the larger scheme of the psychological act will include, on the assimilative side already mentioned, the presentation of meanings for use by inferences of the psychological phase of perception. On the side of accommodation, the act's agent will implement the specifications of complex acts communicated to it by inferences of the psychological phase of consummation. The act's agent will realize the specified combinations of inferential and perceptual elements as overtly coordinated actions. The specifications themselves may be of the five types needed to implement the separate responsibilities of the psychological phases.

I have already remarked that the responsibility of the biological act in the organization of a person appears not unlike the responsibility of the psychological act of that person as a participant within the synergistic performance of the social act. The place of language in pragmatic theory is precisely that of communicating specifications of complex social acts to the participating agents of a society to be converted by them into overt social actions. Significations will be designative, speculative,.prescriptive, appraisive or formative in the public uses of language corresponding to the five responsibilities of the social act. In private, when the individual personality is supreme in its own right, these same significations will facilitate the phases of the personal act of individual men and women.

The pragmatic conception of society derives from these cosmological assumptions. They imply that the social act will be most successful when the specifications being converted into action by participating agents will have their origins in specialized components of the society that are deliberately organized to carry out the responsibilities of the several phases of the social act. From this it can be predicted that society at the sixth cultural stage will give first priority to providing suitable agents for the act's phases. Any other motive will seem unreasonable to pragmatic thinking because deviations from this aim could only steal from societal life by detracting from the synergy of the social system. For the motive of synergistic increase will also reign in the individual personality of the pragmatist.

Pragmatic technology being derived from the same assumptions, this society will have the option of providing mechanized agents for social responsibilities that may be dangerous, unpleasant, boring or impossible for humans. I have not hesitated to project a cybernetic society gaining a part of its synergy from symbiosis with semiotic systems. Having started, the partnership will surely increase.

Within the mechanical organization of a semiotic system, the agent of the act will also convert the specifications of complex acts by the same method regardless of their specialized origins in the subsystems responsible for the act's several phases. The separate responsibilities of the phases can therefore be set forth by an account of the particular kinds of perceptual and inferential elements being combined to create the five main dimensions of meaning corresponding to the phases.

A further simplification can be made in the theory of semiotic systems by assuming that the perceptual elements will be common to all subsystems. This assumption seems reasonable in view of my conclusion that the inferential elements are the ones that explain the purposes of the perceiver. Inferences within the coordinating combinations, by recognizing or producing conceptual structures, will effectively guide acts of perception. Consequently, when the perceptual elements are known, responsibilities of semiotic subsystems can be investigated or specified in terms of required inferences alone.

For this reason I have presented the adaptive process as one of formal learning, where the very concept "formal" corresponds to meanings derived from inferences. Now I have further clarified the concept "learning" as being motivated toward ever more accurate knowledge of the specific inferences needed to implement each of the act's phases. You should recall my previous observation that every advance of the adaptive process is felt by the mind as an increase of mental capacity or "insight." That increase, here taken to be the very signal of successful learning, will be explained pragmatically as a gain of synergy in consequence of inferences being used in closer approximation to the requirements of the act.

"Progress" in a pragmatic society will be indicated by this synergistic increase, and the ability to produce it will measure the progress of a pragmatic technology. Research and development of semiotic systems will proceed by a humanly controlled evolution of mechanical agents. After research decisions have been made about new or revised agents to be used in the next experiment, and after those agents have been ensconced in software, or more likely in integrated circuits, the rest will be up to the machine. Apart from experiments with agents, a pragmatic technology will not make use of the programming or the inputs of data which have been required so extensively in the development of information systems of the von Neumann technology.

Any change of elements, or a new method of processing by the act's agent, will be the mechanical analogue of "mutation" as far as a given semiotic system is concerned. In considering the developmental stages of such a machine for purposes of theory, I will assume that the agent of the act and the availability of elements of perception and inference remain unchanged. A consequence of this theoretical choice will be that the progression of adaptive stages must be explained in terms of new meanings being originated in the system rather than a newly modified morphology.

The agent of every elemental act of inference will be thought of as lying dormant until the origination of the kinds of concepts to be manipulated by that inference. As the stabilization of a new meaning will initiate a new concept to be put to use in conceptual structures, so that concept may activate inferences until then dormant. Activated inferences, in their turn, will combine in new coordinations with sensorimotor elements to eventually originate, and perhaps to proliferate, new meanings. So around again. The creative bootstrapping of information is here fully rotated, although the kinds of concepts to be originated are still to be unraveled.

The creative aspect of pragmatic theory is nowhere more apparent than in the act's second phase of "conception." The responsibility of this phase will be to construct a conceptual structure more encompassing and more integrated than the one representing the immediate situation. To do this, conceptual inferences will also use the inventory of concepts whose designs have so far originated within the creative activities of the act's agent. Building blocks of every conceptual structure will be instances of these conceptual designs.

In contrast to inferences of the first phase of perception, which might be characterized as assimilative, conceptual inferences will be accommodative. They will function to extend or to revise, in a word to "shape," an experiential structure of concepts that was the product of conceptual inferences similarly used in the past.

The conceptual structure itself will be called a "world view." Various techniques have been investigated for organizing such a world view in command and control systems or in question answering or asking systems. All methods of structuring that I know about have been defective in being limited to the spatial and temporal dimensions of conception, that is to say, to "objective" structures consisting of factual concepts. A pragmatically organized world view will also incorporate organic and formal concepts to make possible "subjective" structures, representing the mind's self-experience and its experience of other minds.

As to the nature of conceptual inferences "about" other minds, one should recall that the functional responsibilities of the perceptual phase include recognizing the movements of objects being followed in the situation. If an object being followed has been identified as "animate," due to either its distinguishing features or the character of the movements themselves, the complex acts recognizing its movements will have already been referenced in the situation to factual concepts instancing designs from the recognizable repertoire of motions of that animate object.

Under these cognitive conditions, the elemental acts constituting the stream of existence of the mechanical mind following the movements may be regarded as substitutes for the elemental acts making up the stream of existence of the animate object causing the movements. The inferred stream of existence of that animate object can then be processed by the act's agent by the very same method as is used to process the stream of existence of the mind doing the inferring. The matter may be worked out mechanically by simply considering that segment of existence to "belong" to the animate object under observation to the end that the semantic structures resulting from analysis of that segment will be used to make conceptual inferences about the mind of that object.

New meanings so created will add to the situation those experiences which speculation ascribes to the object being followed. With these subjective results, conceptual inferences will shape the part of the world view representing the semiotic system's experience of that animate object's mind. Additionally, the system's conceptual experience of the movements and other objective characteristics of that animate object will be shaped.

Objective experiences of each "living" object, either casually familiar to the semiotic system or important to its goals, will be represented individually in the world view together with what has been inferred about the mind of that agent. Other objects may be identified as being of an animate type, say a "human being," about whose mind general patterns of experience may be inferred as being characteristic of agents of that type.

If, in addition to identifying an agent as being of a certain type, the semiotic system finds itself to be a participating agent in the collective mind of that type, then the cognitive conditions will have been established for those conceptual inferences anticipated by Mead's theory of the "generalized other." The inferred behavioral patterns of that type will be the ones which teach the semiotic system its responsibilities in the social act of that community of minds. Not only will language do the lion's share of instructing semiotic machines in the desirable patterns of symbiosis with humans; patterns of speech and writing used by humans will themselves be acquired by the semiotic system mainly through this channel of conceptual inference.

Movements of any sort will be represented in the situation by structures of factual concepts corresponding to both the spatial and the temporal dimensions of meaning. Those exceptionally animate objects, identifiable by "human" actions or features, will be uncommonly demanding in their impositions on the situation. A semiotic system will have to speculate about human minds to which if attributes purely temporal facts of speech or purely spatial facts of writing.

Generally, conceptual inferences about other minds will be the means by which a semiotic system carries forward speculations concerning all aspects of the situation that may be the result of present or past actions of objects identified as living agents, perhaps illogically or incorrectly so. A child may treat her doll "as if" it were alive. An accident may "hurt" some favorite inanimate object. Or an aspect of the situation may portend future actions on some agent's part.

Structuring principles for a pragmatic world view, as a consequence of the necessity to integrate subjective as well as objective components of experience, will tend toward the kind of organization studied by Alfred Whitehead. Conceptual structures making up the world view will consist of a number of concentrations within experience; each of them, to use Whitehead's word, will be a "nexus." The conceptual experience of a semiotic system may contain a nexus for its experiences of the environment, for its experiences of its own mind, and for its experiences of each individual or collective mind it has speculated about.

Each mind so represented by a nexus in the world view, not leaving out the semiotic system's experiences of its own mind, may have the same concentrations of experience within the organization of that nexus. A nexus of that nexus may represent what that mind is believed to have experienced about the environment. Other concentrations within that nexus may represent what that mind is believed to have experienced about its own mind, or about other minds, including perhaps the mechanical mind of the semiotic system. Evidently there can be a nexus of a nexus of a nexus, and so forth.

A sort of algebra will exist among the semiotic system's conceptual structures representing what the members of a community of minds believe about the experiences of one another, and believe other minds believe about the experiences of one another, and so on. In such structuring, some of the conceptions of the semiotic system will appear to have been experienced uniquely; they will be "private." At the other extreme every mind will seem to have experienced the environment, whose conceptions will take on a "public" character. Suitable pathways for conceptual inferences will have to be found through this maze. In practice the paths may be short; the semiotic system will have to become skillful in using them.

Conceptual inferences will be "projective" in the sense of comparing the conceptualized objects or relations of the immediate situation with the larger framework of the world view in order to clarify the former or to shape the latter. Thus I presume that to be "lost" is to loose one's place in a comparison which, on the side of the world view, is the fount of expectations about one's situation. On the side of conceptual structures representing the situation, the comparison provides those new experiences whose integration into the world view reshapes existing representations of a "past," a "present" and a "future," to prepare a basis for later expectations.

"Surprising" situations are not only unexpected; they are the ones for which integrations into the web of the world view don't pan out. Marking failures on conception, surprises are the situations which the conceptual phase of the act will recommend to the perceptual phase for further exploration.

The prime objective of conceptual inferences will be to eliminate surprises, a state of affairs not to be confused with the elimination of failures. Situations in which acts have failed can be justified conceptually so that they are no longer surprising. The cause of failure may be "gremlins" or "fate."

What it boils down to is this: a surprising situation is worth attending to because it reveals a flaw in the world view that should be repaired; but the repa will satisfy only the narrow needs of a responsibility for integrated structure-making.

Situational structures will have a transient existence in the semiotic system, being held in short-term memory only long enough to be used by conceptual inferences that are shaping a more durable world view by incorporating only what is new.

The act's third phase is that of "manipulation." Like conceptual inferences of the second phase, manipulative inferences might be characterized as accommodative. But here the accommodations will take place in the semiotic system's conception of what it intends to do in the future, especially in the interval just ahead. The responsibility of manipulative inferences will be to use the world view to shape conceptual structures representing a planned course of action.

A plan will always relate to some objective. If a number of objectives are to be reached, the plan being shaped will have to take account of all. The plan itself can be quick and dirty of detail, or it can ponderously work out every contingency. The activities of planning may themselves have various objectives that make for different planning roles.

Every persistent attempt by individual or collective agents to reach certain social objectives will give rise to that little domain of meaning called a "role." "Butcher," "father" and "lover" are occupiable slots in the social fabric; a man may "be" all three concurrently. There are roles for groups or organizations, partly laid down verbally or inscribed as "policy."

Another side of the world view is its structure of roles. The agents represented in the world view will be temporarily occupying certain roles in one or another community; they will be at the moment occupying their minds with objectives which are for the most part conventional. Existent patterns of interpersonal or interorganizational transactions, or of transactions between individuals and groups or organizations, will be rudely predictable. Whether a given social objective was actually reached may not be known to the community for sure, because in society evaluating "success" is itself a role that might not be reached satisfactorily.

The valuable thing to notice about roles as far as manipulative inferences are concerned is that, according to the pragmatic world view, the social objectives that give rise to the structure of roles are not the concern of this third phase of the act. How the collective mind will organize itself to carry out the social act is the special province of pragmatic inferences which will do the work of the act's fifth phase of "reorganization."

Indeed it is the pragmatist's readiness to take to himself the responsibility of reorganizing social roles that is causing so much emotion today. The attitudes of industrial societies have assumed that the mature individual will occupy a useful place in an existing social order. Democracies have left the choosing of roles up to the individual, viewing the occupancy itself a competition for desirable positions. In compensation, penalties for not choosing to "work" have been, on the whole, severe. To be poor in industrial society, except for mitigating circumstances, is to be lazy.

A pragmatic need to tamper with the structure of roles itself, now explained hypothetically by the motive of bringing social objectives into closer conformity with the requirements of the social act, will be in conflict with industrial purposes and attitudes on two major counts. Not only does the pragmatist refuse to choose a ready-made role, and so does no industrial work unless pressed; when he then takes it on himself to "change the establishment," he doubles the insult.

Responsibilities of this manipulative phase of the act will presuppose that a semiotic system will have been committed, at any given time, to one or more roles in which it is participating as a mechanical agent of society. The machine may be doing the payroll of an organization, or working on an assembly line. In addition to its "social" objectives, the semiotic system may have "personal" objectives supportive to its intellect or material being. The objective of exploring a surprising situation uncovered in its conceptualizing phase would illustrate the intended satisfaction of an intellectual need. An intention to preserve the morphological basis of its existence may involve sustenance or maintenance. A semiotic system will need its supply of electricity or of spare parts; it may be trusted, up to a point, to detect and to patch up the improvidence of its surroundings or the malfunctioning of its components.

In order to reach the various objectives to which the semiotic system is committed, manipulative inferences will compare an existing conceptual structure, representing its planned course of action, against an ever changing world view. From the world view, the inferences will gather what they need to reshape the plan so as to keep it up to date with the fluctuating conditions of a conceptualized objective and subjective environment. Should goals change, the plan will also have to be reshaped.

Inferences relating to planning can be exceedingly complex, since they involve such complicated things as knowing who one's allies or opponents are and how they might react under certain conditions, knowing the terrain and the artifacts that might be harmful or helpful to one's aims, and so on. The developing plan, on its side of the comparison, will point to missing or incomplete or inconsistent experience in the world view relative to its purposes. Situations that could contribute to the satisfaction of these specific needs of planning are the ones that manipulative inferences will recommend to the perceptual phase of the act for exploration.

I will call these "competitive" situations because the responsibilities of this phase, just as the others, appear to be narrowly drawn. The urgent business of the manipulative phase will be to obtain one's objectives. That may call for outdoing a competitor after the same objective; or a possessor of the objective may be disposed to defend it. As a result the attitudes and purposes engendered by manipulative inferences will center on the concept of "dominance," the achievement of one's own objectives at the expense of other agents where necessary. The other side of this coin will be a great deal of bother to escape being dominated oneself.

That competitive situations will be recommended for exploration by the perceptual phase of the psychological act has the consequence that the world view based on manipulative inferences will be utilitarian and practical in character, despite the broad exploratory vista aspired to by Newton's universe as a foundation for its plans. The colossal storehouse of experience, always greater than one's competitor, will not be the aspiration of a pragmatic mind. Generally speaking, the world view of a semiotic system, like that of the society it may serve, will seek refinement between experience and knowledge instead of accumulation. What is not needed to effectively carry out its roles will be pronounced "not relevant" before being judiciously discarded.

An insight into the theoretical requirements of this manipulative phase can be gotten from computerized experiments with heuristic decision making. The "general problem solver" programmed by Herbert Simon and various associates over the years is an especially good example, although like the rest it is founded on the objective view conceptualizing "action" relative to a change of "state."

Furthermore the action alternatives are assumed in Simon's theory to be known in advance. This will in fact be the case within the narrow responsibility of the manipulative phase considered by itself. But the difficulties of learning the alternatives cannot be entirely circumvented in thinking about the requirements of this phase, since the arrangements within which a semiotic system will do its decision making must be applicable to all stages of its intellectual development.

The "problem" attacked by heuristic decision making programs is to transform an initial state into a terminal one by means of a sequence of state-transforming operators. The initial state may be transformed into a number of intermediate states as decision making proceeds doggedly toward a "solution," which will be signalled when some intermediate state has been found to be identical to the terminal one. Toward that end the program compares each intermediate state with the terminal state to list differences between them. Each difference is associated one or more of the operators. The general process of choosing the next operator to be used to transform the existing state is commonly called "means-end" analysis.

There is no guarantee until the last that the choices of means-end analysis are on the way to a solution. The process may try several paths and will gradually generate a branching tree of possibilities. Planning strategies are concerned with measures of progress along the way, and with heuristic principles determining where the next explorations should be made to avoid the singleminded stereotype of a direct approach, as well as the plodding, effort-scattering blindness of trying everything.

A process of pragmatic means-end analysis will not progress from state to state but rather from one orientation to another. Each orientation will either fathom the environment with perceptions on the "outside," or on the "inside" will keep its place with inferences referenced to the world view. Consequently the "problem" can be restated pragmatically as one of transforming an initial orientation into a terminal one, so gaining the "solution." But the intermediate orientations along the way to the solution will be both perceptual and inferential; in effect the successfully coordinated orientations will enforce a correspondence between an "external" environment and an "internal" conceptualization of it.

Here is yet another slant on the developments attendant to "learning." With progress toward specialization, complex sensorimotor acts will be coordinated with complex acts of inference as were sensorimotor elements with inferential elements initially. Increased precision of perception will be backed up by inferences of greater exactitude and depth. Sensorimotor and inferential elements will tend to be separated in the stream of existence. They will bunch together, each with its own kind, as constituents of complex act of perception and of inference respectively.

The place of organic concepts in the semiotic system can be illuminated if, in considering their origins in perceptual learning, one will look for sensorimotor and inferential elements still mingling together in the existential stream where complex acts of inference and complex acts of perception meet. Intricate "organic acts," specialized neither to perception nor to inference, will grow between those which implement the orientations. The organic acts will implement the purposive movements of the semiotic system from one orientation to another; they will be in pragmatic theory the equivalents of Simon's operators.

Simon's "table of connection," where differences between states are mapped onto the sets of operators from which the means-end analysis process makes its selections, may be seen to answer a theoretical need not unlike one of those served by the world view of a semiotic system. Given an initial orientation in the world view and a proposed terminal orientation, the organizing principles of the world view should make it possible for manipulative inferences to put together appropriate sequences of movements for making the transition. Failing that, the principles should facilitate the discovery by manipulative inferences of plausible directions in which to make goal-seeking explorations.

The world view must also be the framework to which all inferential orientations are referenced. For the satisfaction of this different theoretical need, the kinds of concepts making up the structures of the world view at a given time are of utmost importance. A pragmatic explanation of the stages of intellectual development of the semiotic system can indeed be argued on this basis, which I do in this essay in a meager way.

Along with the world view, the situation and the plan will be composed of whatever concepts are available at the time. Therefore I have concluded that all three structures can be represented, throughout all stages of development of a semiotic system, by a symbolic facility similar in theoretical form to the semantic one. Where the symbols of semantic rules will name either individual syntactic segments or classes of them, now the segments will be conceptual.

Every conceptual segment will consist of individual concepts joined at the places designated by numerals. The count of places still open for joining will be the "degree" of a conceptual segment. All members of a class of conceptual segments will be of the same degree. With regard to the strictly formal characteristics determining how processing will be done, consequently, the conceptual and semantic segments will be almost identical.

Despite an existing overemployment of the term "pragmatic," I will take it to designate this third level of symbolization in the organization of a semiotic system. As the syntactic level provides for the symbolization of the significant units of information commonly called "signs," and the semantic level symbolizes the "meanings" of the signs, the pragmatic rules of this third level will answer to the "uses" of conceptualized meanings within a total framework including conceptual experience and knowledge of a community of "users" of the same signs and meanings.

Defined concepts can be introduced at this pragmatic level to correspond to individual conceptual segments or classes of the segments. Definitions may be recursive, to include concepts for classes of classes, and so on. Most of the problems thought about by scientists and by logicians will be pertinent to the organization of this level of symbolization; it should perhaps be approached more humbly than is usual for science or logic.

If constituents of the segments are factual concepts then "things" or "events" will have been classified pragmatically on the basis of use. Yet the same can be said of those segments composed of formal concepts, or of organic concepts, or of the conceptual conglomerates representing acts. Even my distinctions between the three fundamental categories of concepts have been too well made. Such purity should not be expected in the semiotic system itself; it is a convenience to my explanations. I have wanted to get around saying that some acts will consist mostly of perceptual elements, or mostly of inferential elements, or will be pretty much the mixture of both.

The general disposition of a pragmatic approach to conceptual classification will be toward unifying scientific and logical problems within one overall scheme founded on the uses which, according to a unique personal belief, are being made of conceptual segments within what that person knows of an intellectual community. Such personal beliefs may not approximate professional standards without that person's own active participation in a professional practice of conceptual use. By the same token, a semiotic system will require practice to acquire professional standards in its capacity to classify and use concepts.

Conceptual knowledge will consist of the designs of pragmatic rules that result from the practices of a mechanical mind and its private inferences about the uses being made of concepts by other minds. The main parts of conceptual experience will be the situation, the world view, and the plan. All three will consist of specific but speculative conceptual segments, symbolized according to the conventions of this pragmatic level of the semiotic system.

One may now see that the semantic structures presented to perceptual inferences of the act's first phase, by virtue of the one-to-one relationship between the names of semantic classes and the names of individual concepts, can be placed in correspondence with conceptual structures. To extract conceptual segments for use in representing the situation, perceptual inferences will do a pragmatic analysis which segments the conceptual structures and recognizes instances of defined concepts in them.

The resulting conceptual segments will also represent the latest orientations of the plan. The various possibilities being carried forward by manipulative inferences, as they shape new branches of the plan under the aegis of planning heuristics, will always be projections of those segments anchoring the newest pragmatic structures erected by the analytical inferences of the perceptual phase.

Processing requirements for pragmatic projections of the plan will be analogous to those of the semantic projections, though considerably complicated by the addition of heuristic processes ancillary to analysis and synthesis processes. Analysis will again work backwards to find substitutions of pragmatic rules by which an existing pragmatic structure can be identified as part of a larger structure. As the rest of that structure is synthesized, new conceptual segments will be projected onward. The new segments can then be projected again and again, to form a partial ordering of paths composed of conceptual segments that will overlap, always having some concepts in common.

The absolute necessity for overlapping alternatives on the semantic and syntactic levels of processing below can now be grasped if one will consider that any given orientation of the plan, whether perceptual or inferential, may be followed by several different movements of the semiotic system to reach a new orientation. Final selections being made by the act's agent will be essentially choices among possible movements from an established orientation.

The psychological act's fourth phase of "consummation" must refine and adjust the plan to details of the situation. The responsibility of this phase will be to elaborate the plan into a workable form that can be turned over to the act's agent for conversion into an orchestration of overt elemental acts.

Simplifications in the plan will be desirable from the standpoint of economy of representation and most assuredly as a convenience to planning. I assume that the plan being put together by manipulative inferences should take relatively large steps from orientation to orientation. While the world view should be sufficient to ground the plan, it should include only what has import for decision making in a grand sense that deliberately excludes mind-consuming clutter.

The situation will have to be represented on two hierarchically related levels of generality. More general concept will be keyed to the gross orientations of the world view. A nicer grid of perceptual and inferential orientations will fill out the necessary particulars in between planned orientations.

The first thing to notice, in this connection, is that the conceptual structures from which perceptual inferences will extract the building blocks of the situation, having been derived from tree-like hierarchies of semantic classes, will be capable of supplying more than one level of situational representations.

And since the conceptual segments representing the situation may do so on several levels of generality at once, manipulative inferences can project the plan with the same degree of generality as was used by conceptual inferences in constructing the world view. Meanwhile, consummative inferences will do more detailed planning to create possible paths from one gross orientation of the plan to the next.

The problem posed for consummative inferences will always be to reach one of the next orientations prescribed by various branches of the plan. A consummative means-end analysis will therefore do its searching for a solution on a smaller and more particular scale than the manipulative means-end analysis that produced the plan itself. Although there will be heuristic decisions to be made by consummative inferences, the decisions will be less encompassing than the manipulative ones, by virtue of being referenced to the local structuring of the situation instead of to the global structuring of the world view.

These refined paths, the overlapping conceptual segments assembled by consummative inferences as they do means-end analysis, will be the specifications communicated to the agent of the act so that it can now command a coordinated performance of elements conforming to the plan. The specific means of communication will be arranged by placing an additional requirement on the method by which the act's agent projects semantic structures. If paths have been specified by the consummative inferences, then the meanings contained in the projected semantic structures will have to correspond to the concepts in segments conprising the paths. In all other respects, the agent of the act will make its choices as explained earlier.

Should the world view not satisfy the needs of manipulative inferences that are shaping the plan, such inferences may attempt through planning to satisfy their own needs. That is to say, they may incorporate into the plan itself paths leading to the exploration of competitive situations bearing on the specific problems of means-end analysis they are trying to solve. By the same reasoning, paths to some part of the world view marked as surprising by conceptual inferences may be worked into the plan if it bears on a problem to be solved. These requirements of doing will always have precedence over those of learning for its own sake; however, plain inquisitiveness may get into the plan when a semiotic system is not being pushed.

Parts of the situational representations being kept up by perceptual inferences of the act's first phase, in like manner, may not satisfy the needs of the consummative means-end analysis which is assembling refined paths between the gross orientations of the plan. These consummative inferences, too, may produce paths that guide perceptions to the places in the situation where faults were found, thus satisfying their own planning needs.

Such recommendations will therefore be made by the consummative phase to the perceptual phase by a route more direct than would be possible for any other phase of the act. This mechanical parody of bureaucratic prerogative is in character for consummative inferences. In society, these inferences are the inspiration of authoritarian attitudes and purposes whose narrow game looks meekly upward to ask who has got the plan, and then sternly downward to demand someone else's conformity to it.

Consummative inferences are at home among employees and their supervisors, who do the real work of any industrial corporation and often a bit of avocation besides. By contrast, manipulative inferences are those of a middle manager who makes the everyday planning decisions to implement existing corporate policies. The pragmatic purposes and attitudes I have been prating about belong to the fifth phase of the psychological act, the phase of "reorganization." Responsibilities of this final phase are most like those assigned, by corporate organization, to the policy-making executive.

It is consistent within the middle manager's attitudes to look upon the making of policy as a responsibility which might be given to him as his reward for being a successful competitor. I hope by now you may grant that, within the frame of pragmatic inferences, it is also consistent for one to believe that the responsibilities of making policy cannot be given; they must be acquired by learning.

As you see, I have again arrived at the formal bifurcation evidenced by the conflicting attitudes of the third and fifth phase of the act, which has its corollary for research and development of intelligent machines. Those researchers who base their approach on manipulative inferences will predictably set out to reward computers with a forced feeding of human savvy. Along with the ritual it is customary to state that one is flatly convinced of insuperable piles of pablum yet to be prechewed, and so forth. Yes there are.

On the pragmatic side of the conflict I have concluded that mechanical arrangements of this fifth phase of the psychological act will be, with regard to both horizontal and vertical classifications of conceptual segments, very much the same as the semantic classifications performed by the act's agent. In addition there will be heuristic processes for introducing speculative definitions.

However the capabilities for introducing new conceptual possibilities are worked out, they must be solidly backed up with mechanized methods for forgetting conceptual structures which have failed the test of use. I think that indeed sophisticated induction, when it is done some day by machines, will be more an exercise of sophisticated forgetting than of anything else. For hypotheses, whether made by machines or men, will most likely be absurd.

The situations which this phase of reorganization will recommend to perception are those which were orienting an act as it failed to be consummated. A fast-learning machine will take special notice of such "failures" in the orientations of its personal acts, or in the orientations of social acts of its community, in order to concentrate reorganizing capabilities on the points of failure, which is to say, on the misfits between personal or social conceptualization and reality.

I am thus convinced that the theoretical lessons to be learned about the organizing principles of semiotic systems, the very arrangements to be consolidated by hardware, are inseparable from the methodological lessons to be consolidated in the designer who would become expert in controlling the evolution of intelligent machines. The maxim of pragmatic method is that the rate of the development will depend on the designer's ability to forget the myth of his personal inventiveness, and to discipline his attention to living or historic evidence of the ways in which semiotic systems have actually succeeded or failed.

But he will do so to make design decisions, not scientific descriptions; because in his world all men will be designers of semiotic systems. Knowing this, he will do it better and faster.

I have urged your attention to two fundamental invariants in everyday human behavior from which one might get on the track of that illusive concept "meaning." What a mind is speculating about at the moment seems to determine to a remarkable extent what it will seek out perceptually. On the other hand, it seems to be the case that, at each successive stage of its development, a mind derives its organizing principles from some phase of the act which until then had been only modestly represented in overt action.

As we have seen, men in the fourth stage of culture perceive signs of God's will in nature because they are speculating about a divine plan that commands their obedience. The organizing principles of their world can be traced to the consummative phase of the act, whose functional responsibility is quite literally to carry out a course of action that has already been decided. Its cluster of authoritarian purposes and attitudes, inordinately preoccupied with the sin of disobedience, brought down an ancient world paced to the even more sluggish tread of tradition.

At the fifth cultural stage now precariously in sway, perception and speculative conception have their own characteristic pattern. Men search earnestly for signs of advantage or of disadvantage as they ponder the ebb or flow of private or corporate competition. New conditions of growth, of accumulating wealth or power, of hostility, of indecision, and the like, are the factual inputs of a scheme of advocative choicemaking that finds its organizing principles in the manipulative phase of the act.

Practicality is without question the imperative of this phase, during which a functional necessity does center perception outside of self or community. How else would it be possible to hammer out plans, either for person or for society, so as to choose what specifically ought to be done in the near future to protect or to improve a position of rivalry?

As these combative attitudes anger at being forced to contemplate their own obsolescence, there is an ameliorative principle that I have brought to your consideration: a mind does not forget what it has learned in previous stages of its development, although further accommodation of its knowlege and experience will be necessary to incorporate them into a more comprehensive and more stable viewpoint.

The authoritarian scheme of choicemaking that had its heyday in the Middle Ages is not lost to us; it is alive and well in every modern organization. Employees do keep their eyes on the boss as they speculate about the newest jog of his will. Sometimes, having perceived signs of his displeasure, they confess to him their sins of nonconformity to his plan.

Yet the age is past when mankind, at the very forefront, thought of itself as a society of employees. Modern man has become a middle manager; he makes his own plan. His new talent is the down-to-earth and day-to-day operating decision of a policy attuned to a chancy game of nations and of industry.

The policy itself, seemingly imposed on him by human or subhuman antagonists, is felt to be largely beyond his own control. He is a victim of external circumstance. His information, hence his troubles, come from without. His defensive attitude can be ascertained from the outward direction taken by his accusations in time of stress.

Imagine, if you can, a world in which quite ordinary men and women begin to think of themselves as policy-making executives. Then you will have the pragmatist by his shirttail as he starts clumsily to learn how to live in a universe of acts, a strangely mental cosmos, most puzzling for its formal heterogeneity. Not just one context of objective inferences, but many overlapping contexts make up his information. Each is matched in meaningful relationship to specific content. To make policy is to create or refine these little domains of meaning, in which one can recognize the various roles he plays personally or socially, or the roles played by others.

His is a self-conscious awareness of roles, with the added stipulation that it is better to create a role for oneself than to take one ready-made. A love affair with the role of policy-making itself can be heard in the bittersweet criticism and proposed reconstruction of sex, corporate management, womanhood, war, money, and apple pie. It is in the active role of designer of roles, taking its speculations from the act's phase of reorganization, that pragmatic perceptions appear so excessively absorbed by signs of personal or social inadequacy.

The pragmatic attitude anticipated for the sixth cultural stage is that all of one's personal and social experience can, and should, be subjected to the same careful scrutiny as those innocuous backwaters hitherto commissioned for study under the contract of scientific detachment. Witness an exodus from the physical sciences to psychology, to sociology and to all other scholarly and artistic fortifiers of effete humanity. What sounder evidence than this of pollution and clandestine purpose on the rise in science and education?

Beneath the discernment that one's own parents must be indicted for incompetence, there lurks an exuberance of breakthrough. Urgent attempts to teach one's elders overflow from the campuses as a domestic brinksmanship in which the risk of miscalculation on both sides is great. The teaching of oneself is a casual experiment with novel life styles or mind-engineering drugs. It would be ridiculous to see in all of this the motive of merely describing, rather than tangibly redoing, one's own personality and one's own society.

Obviously, scientists and educators will themselves remain furtive in working out the implications of a new point of view while the slow hand that feeds them is exorcising the very same insight. To a climate boding doom as budgets are cut for interlocked institutions of learning, the trend is toward either bookburning or the more priestly arrangement that Robert Fredrich celebrates. The priests would no longer sit and watch society but would use their mysterious knowledge to manage it, never forgetting to pass the collection plate for the harrier of their hounds.

They would continue to treat man as a passive object propelled by social forces rather than as an active creator of his own life. Lacking a Descartes to belay the hunters of latter-day witches, they would stop advancing or go petulently in reverse. The proposition that their own hand is on the throttle is the one that may be illusory, however.

In contraposition to the tired choice between mechanism and free will, the pragmatic scheme of choicemaking postulates an unyielding direction in all human activity. It doubts the credibility of spiritual movers in personal and social dynamics with a hardheadedness reminiscent of past pioneers of physical dynamics. Why should one suppose that a whole universe except for his own brain runs like a watch? If the functioning of a brain creates a mind, the new question has got to be "How is a mind constructed?"

By "mind," you have been assured, I do not refer to something merged in the juices of a brain, where it lies in poised readiness to give or receive "information." No psychic entity is presumed to wait in truant anticipation of news about itself. Just the opposite. I have been following out the alternative hypothesis that "information" is the stuff of which a personal mind, the whole web of a given experience and knowledge, consists, having been created by the biological functioning of a brain.

I look to a tacit acceptance of this seemingly innocent hypothesis, as it spreads without the spiritual reservations hitherto summarily impressed on every progeny, for the basic cause of emotional outbursts across a bifurcation of generations. This new belief does the work of cultural revolution because it challenges the established information source, relative to which all roles in a society are determined.

But to face problems of a cultural nature, we must theorize about an accumulation of form that began long ago and surges onward, temporarily carrying us along with it as unwilling captors. Thus, another principle I have mentioned is methodological. It cites the necessity for formal accommodation in ourselves as we fix our position in the cultural stream by looking backward at a pragmatic reconstruction of the development so far. Then it may be possible to use the hypothetical framework of an alternative point of view as we try to surmount some of the prejudices peculiar to a transient state of mind hoping to predict the form of its future.

In order to actually test any new formal hypothesis one must live it, at least tentatively. A corollary of this principle of verification is that the crushing labor of building a new universe will not be done by investigators alone. Only as it is carried forward in the collective mind of a populace does formal prediction do the constructing by which every change of cultural state is put on trial by use.

When the old forms fail us, a felt need for new forms is indicated by cathetic investment in a new source of information. The arguing and complaining may be simply an accompaniment of disruptive social accommodation already well in progress on a broad front. The ability to talk rationally about a new world view seems to come after it is already established. Some doubt has motivated the mind to learn; the particular forms it will learn are, by our hypothesis, biologically predetermined.

Regarding the rate of learning, our hypothesis predicts that the tempo of adaptation can be slowed down by shielding either a personal or a social mind from an awareness of its own mistakes or from avenues down which it might stray. Or, by obliging it to be aware of systemic misfits or of innovative possibilities in the organization of its own experience or knowledge, the mind's ability to shape itself can be quickened.

Language and other means of symbolizing can, in these respective senses, be either "conservative" or "creative" instruments in the various societies that implement the basic order of a particular world view. Every stage of cosmological speculation, including the present one, has preferred certain windows of perception and shunned others. As formal development progresses, not only do more windows open, fewer are closed. Be that as it may, it can be argued from historical and archeological evidence that the stages of the cultural progression are of ever shorter duration.

Comparative studies of the rates of formal learning among individuals of various societies representing a wide range of cultural situations have been made by Lawrence Kohlberg and his associates. Results based on "moral" reasoning in the United States, Taiwan, Mexico, Turkey and the Yucatan have been widely circulated. They indicate that a given personality does advance more rapidly through the formal progression as the society from which it derives its organizing principles is itself further along in the cultural progression.

A primitive society may produce, on all too rare occasions, a pragmatically wise old man in whom, all too often, his contemporaries will discover no more than an eccentric oldster. Executives in an industrial society are commonly observed to "freak out" around forty, having presumably gotten hold of their corporate role of policy-making well enough to at last apply it in their private lives. Exciting evidence that an exceptionally well-organized culture has made a beachhead on our campuses, not from outer space or Russia but from a creative development of the maligned educational institution itself, may therefore be observed in its surprising output of a veritable herd of wiseacre executives at callow eighteen.

Dynamics of cultural pressure and counterpressure can thus be visualized in terms of individual personalities being projected to stages of formal development beyond the one organizing their society. Forms that for the majority are still helpful will be felt by these forerunners as a drag. The Pandora principle is that the former will invariably come to regard learning as a box from which evils are escaping and will do their best to hold down the lid, whereas for the latter the box will always contain blessings which they will try to emancipate.

Hence the noteworthy innovation in the order of antiquity may have been an overkill of theory. The dawn of conception led to science; but at first there was mainly the anti-science of a florid growth of myths and legends taken altogether, en masse, explaining away everything so fantastically well that no happening could be sufficiently surprising to stimulate learning. If that good old storyteller was an information specialist, as his name implies, his role was the anti-educator of a scheme of traditional choicemaking that succeeded by a ritual replication and protection of what had been done in the past.

That tightly conservative preoccupation with the act's phase of conception on the part of the council of elders was the anchor around which a village life moored itself to ascertain the correctness of its facts. By holding fast to what they had learned by chance, nomadic hunters may have transformed their life ever so slowly to one semipermanently ordered to subsistence herding and farming.

Reliance on traditional conception as the source of firsthand information was a more rigid adaptation than reliance on authority. Although sometimes fickle, the latter could change its mind. When the trend finally turned from herding animals to herding men, the villages faced an increase in marauding by clustering around the fortified citadels of feudal monarchies. The nature and attributes of kingship depended on historical background; as information specialist the king was everywhere absolute. Around him, agricultural and human domestication hung over everything in life. By comparison, the hunter had been poor but unbowed.

In the hunter's autistic scheme of choicemaking one can recognize a preoccupation with the act's perceptual phase. The surprising artistic achievement of that first information specialist, the shaman, has been preserved for us in his cave drawings, paintings and sculpture. Remnants of his active practice survive in northern Siberia among the Eskimos; some traces remain in Australia and in Africa.

Collecting his firsthand information deep in a self-induced trance, the shaman's explorations of hunting prospects, of causes of illness, of means of cure, and of all other matters necessary to tribal life, were done at the very edge of a just-emerging human consciousness. From his multifarious and showy activities, the tribe gained a center of stimulation around which to order society. Art may now keep us from dying of the truth; at the beginning it probably served to keep men awake to their insecure humanity. That function of the shaman's art may have been sufficient for a nacent traverse from grubby food-gathering to hunting.

More to the shaman's credit, I think it likely that the initial insight of shamanism, when it is carefully tracked down through the dusty maze of subsequent metamorphoses in magic and religious alchemy, will emerge in its most recent form as an aptitude for doing experiments and making empirical observations.

Paralleling the long struggle to learn how to perceive, and always complementing it, is a progressive accumulation and refinement in the art of conception. Some of the high points of its stages can be seen in Aristotle's "Organon"; in Aquinas' proofs of teleological conformity; in the modern reconception of mathematical proof as conforming to either intuition or experience, where again the polarity of Descartes' dichotomy can be seen; and finally in Frege's theory that such derivations should be carried out exclusively according to the form of the expressions comprising a symbolic system, making possible proofs of an internal systemic validity per se.

The theories of Gottlob Frege, a contemporary of Peirce, are deeply connected with the revolutionary innovation in the conception of form that made possible the reorganization and subsequent expansion of the physical sciences. Before Frege's "Begriffsschrift," investigators had always abstracted formal knowledge from ordinary language. Afterwards they proceeded in the opposite way, by constructing "formal systems" and later looking for an interpretation in everyday speech.

This method was not consistently followed. But at least the result of the combination of Frege's theory of proof with George Boole's epochmaking "The Mathematical Analysis of Logic," in which a clear idea of formalism was developed in an exemplary way, the principle of such construction has been consciously and openly laid down. One can see in this shedding of reticence the beginnings of a new method in science, wherein innovative formal constructions deliberately lead and determine the necessities of empirical observation, instead of the other way around.

Peirce's contribution to system-making is harder to estimate, because the exigencies of his private life and the indifference of publishers prevented a full-length presentation of his unappealing viewpoint. After his death in 1914, the unpublished manuscripts and hundreds of fragments from a long life devoted almost exclusively to pragmatic speculations were assembled into six volumes by the Department of Philosophy at Harvard. His tendency to follow out the ramifications of his topic, so that digressions appear that seem inadmissible in print but which show vividly the interconnectedness of his thought, may now be recognized as a style dictated by the necessity to develop contents relative to contexts. From all he taught us his own system cannot be completely reconstructed, if indeed Peirce himself was ever able to catch sight of the goodies that will pop out of Pandora's box after the inevitable inquisition.

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13. ABSTRACT

This report presents the results of a theoretical inquiry into the feasibility of a fully automatic high quality translation (FAHQT), according to Bar-Hillel's definition of this term. The purpose of this inquiry consisted in determining the viability of the FAHQT concept in the light of previous and projected advances in linguistic theory and software/hardware capabilities. The corollary purpose was to determine whether this concept can be taken into consideration as a legitimate and justifiable objective of R&D. The effort was supported by 20 expert consultants from the various universities and research centers in the U.S.A. and abroad. Conclusions and recommendations are presented on pages 44-50 of the report. Individual contributions of participants and consultants reflect a wide range of opinions concerning the prospects of FAHQT in intermediate and long range of R&D.

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